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THE INTERPRETATION OF GEOPHYSICAL DATA*

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ABSTRACT

Geophysical data result from measurements of physical properties. The geophysicist postulates certain possible physical causes of the observed effects. The geologist reasons from observed geologic effects to geologic causes. The difficulties confronting the interpetation of physical effects as reflected in geophysical data in terms of geologic causes are pointed out. The author takes the position that geophysical data must be worked up independently. A competent geophysicist-geologist may combine geophysical and geologic information in the preparation of a report which will then, and only when this procedure is followed, include all present knowledge of the area under investigation.

The requirement that geophysical data be immediately translatable into geologic language and furnish material for drawing geologic contour maps is shown to be incompatible with the nature of geophysical data. Attention is drawn to the changing geophysical scene and to the tendency to use geophysical methods after they have ceased to be adequate for the solution of prospecting problems.

It is suggested that geology can aid geophysics principally through library reconnaissance and advance surface mapping. Geophysics has become a serious competitor of geology in the search for oil, and the geophysic-geological ecotone has advanced steadily into geologic territory.

"This is a part of the great problem—the problem of geophysics and the problem of all prospecting and geology—the problem of interpretation. The solution is not in sight but it behooves the geologist to work earnestly and sympathetically with the physicist to the end that their results may be translated, as exactly as possible into structure maps—into the language that the oil producer and executive have already learned to read." E. L. DE GOLYER, *Economic Geology*, vol. 23, 1928, pp. 681–82.

It is the purpose of geophysical and geological investigations to learn more about the structure of the earth's crust; in commercial work, to find structures favorable to the accumulation and retention of oil or other minerals, or to locate these deposits directly. In the

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final analysis, information obtained from geophysical measurements is used to supplement existing geological information. The question naturally arises whether the interpretation of geophysical data should be independent of geology or colored by what is known or surmised of the geology of the area investigated.

It may be assumed that most geophysical work is done in areas where the geological information is not complete; it may also be assumed that much geophysical work yields results which are interpretable to greater depths than have been reached by the drill.

Much has been said and written about the interpretation of geophysical data; in these discussions the underlying assumption, the validity of which has been neither questioned nor investigated, has been that something can be added to the results of geophysical measurements which will render the data more directly expressible in terms of geological nomenclature. It has been further assumed that interpretation in terms of geology must be done simultaneously with the computation of the geophysical results, so that the interpreting geologist guides the geophysicist and aids him in the solution of problems as these arise. I refer you to an article by O. L. Brace on "The Interrelationship of Geology and Geophysics," Bulletin, American Association of Petroleum Geologists, February, 1937.

Geophysical data result from geophysical measurements of certain physical properties; the geophysicist endeavors to learn the physical causes underlying, or responsible for, the observed effects. The geologic effects observed by the geologist are due to geologic causes (fundamentally, of course, there are no geologic causes, only physical, chemical, biological); it is the function of the geologist to correlate the observed effects and to deduce the geologic causes.

It may be assumed, and experience has frequently borne out the assumption, that geophysicists are less reluctant to ascribe the same cause to identical effects observed in different areas, than geologists who are guided mostly by similarities in the observed effects. Thus geophysicists postulated faulting in the Gulf Coast sediments long before faults were observed and reaped the mild ridicule of geologists who claimed that "these sediments don't break, they bend." Quite contrarily, sediments behave like plastics and, therefore, always break under tension, as for instance over a deep salt dome. Geophysicists are thus less reluctant to produce "unorthodox" pictures and are probably for the same reason destined to replace the wild-catter whose function it is, according to Pogue, Bulletin, American Associa-

tion of Petroleum Geologists, February, 1937, p. 158, "to bring into view the 'unorthodox occurrences.' " To the extent that the geologist is guided entirely, in the interpretation of his data, by what is known or believed to be possible, to that extent he must rely upon the geophysicist or even on the wild-catter to uncover such "unorthodox" occurrences.

The interpretation of geophysical data in terms of geologic causes requires an unbiased mind which is ready to accept the data at face value and to make those assumptions regarding causes which the observed effects seem to demand whether these assumptions are in accordance with geology as it is known or not. It is a long and arduous road from observed physical effects to geologic causes.

May we also inquire what the field geologist and his brother, the office geologist, conceived by O. L. Brace, are supposed to do to the geophysical data? Will they be able to deduce the anomalous sand conditions at the Smackover field in Arkansas from the geophysical data? They will not; under the conditions given by Brace they will work out a depression from reflection data. Moreover, in order to be able to make corrections, they must have two wells, one on the structure and one off, and how can they locate the well on the structure if the data do not indicate it? Even wild-catters do not like to drill depressions. Or are these geologists to take the geophysical data and use them, correct them, reject some of them, at will and work out a picture in conformance with geologic knowledge? It behooves the geologic profession to take stock and call to mind the prospects which were missed because pertinent data were disregarded; if data were treated with more respect it would not so often happen that the rival company's oil field is found in the files. And why? Because the area was worked, it stands revealed; the data showed the anomaly (as indeed they must) but some geologist drew an *acceptable* picture. I call your attention to the fact that a dozen or more wells were required to impress upon geologists the importance of the faulting associated with the structure in the Mexia-Groesbeck district. "Structure of Typical American Oil Fields," vol II, p. 309.

Before proceeding with the discussion, it is desirable to review briefly the nature of the data which are obtained in geophysical prospecting.

The magnetometer measures the variations of one or more components of the earth's magnetic field over the area investigated. The results are as a rule plotted and lines of equal intensity are drawn. If

a magnetic anomaly is revealed it may, or may not, be possible to draw conclusions as to its structural causes on the basis of information obtained from measurements over an area of known subsurface. If the anomaly is different from that on a drilled area, little can be done; if it is similar, the structure may, or may not, be similar to the known structure.

Data obtained by the torsion balance, the gravity meter, or the pendulum are also plotted and isogams, or lines of equal gravitational acceleration, are drawn. If an anomaly exists, it may be a gravitational high or low and may be due to an anticline or syncline, shallow dome with or without cap rock, or a deep seated dome, in which case the presence or absence of a cap would probably make little difference and a gravitational minimum would be observed.

In refraction shooting, times and distances are measured; from these data, velocities and depths to refracting horizons are computed. In reflection shooting, also, times and distances are measured. Vertical average velocities are determined either independently by well shooting, by refraction work, or, in the case of clear and sharp reflections, from the reflection data themselves. Probable depths to the reflecting layers are then calculated on the basis of assumptions as to wave-paths, velocity discontinuities, absence of multiple reflections, etc., which are not known to be valid or tenable, which cannot always be further investigated, and which may probably prove rather elusive upon further study on account of the numerous difficulties, such as inaccessibility of different layers, multiplicity of acoustic horizons, to mention only two, which are inherent in the problem.

In the electrical methods, potential distributions, transients, or field strengths are measured and anomalies in these quantities are ascribed to anomalies of geologic structure. All problems arising here have, however, an infinite number of solutions, as in the case of other geophysical methods which yield data amenable to computation.

When working according to radioactive methods, it is usual to measure the rate of discharge of the measuring instrument, the electrometer, after pumping soil gases from shallow holes into the instrument. Increased radioactive content of the soil gas is then sometimes ascribed to the presence of crude oil which is known to be an absorber of radioactive substances; it has been found, however, that the source of the radioactivity is in most cases in the surface layers and that radioactive highs cannot be correlated with oil bearing structures.

There is one method which is based on the presence of oil or coal

deposits; soil gas samples or soil are taken from shallow auger holes and are analyzed either in the field or in the laboratory for methane and heavier hydrocarbons. The presence of ethane or heavier hydrocarbon gases is assumed to be indicative of the presence of oil or coal occurrences.

Briefly, geophysical measurements yield the variations of some quantity, the magnetic field intensity, gravity, seismic velocity, time of propagation to a reflecting horizon, electrical transient, potential distribution, field intensity, radioactivity or heavy hydrocarbon content over an area; they yield no direct geological information, but they lead the geophysicist to postulate certain possible *physical* causes of the observed effects. However, the *geological* causes underlying the observed effects are not always understood by the geophysicist and are the subjects of intensive investigations. Certainly, the average geologist's training in the exact sciences is not such as would enable him to extrapolate authoritatively from the physical observations to the geological conditions.

Let us be a little more specific with regard to the data furnished by the geophysicist to the interpreting geologist.

Torsion balance readings yield gradients and curvatures, and there is only one correct way to derive these from the torsion balance readings. If I understand the function of the interpreting geologist, his problem consists of using the gradients and curvature values to compile a map which shows geologic structure. The usual way is to look askance at the gradient and curvature map and recommend leasing the area to which the heads of the gradient arrows point, if a gravity maximum seems desirable, or the area from which the gradient arrows seem to be running, in the case of a minimum. If more intensive work seems desirable, the interpreter first rejects those gradients which point in the "wrong" direction and then finds the isogams corresponding to the vectors which were retained. It seems obvious to me that thus far in the interpretation of the data the geologist was needed only in the rejection operation which to most physicists would be exceedingly painful, because they have been trained to apply strict criteria in the rejection of pertinent data. After the isogams have been drawn, the remaining problem is the deducing of the geologic structure. Physicists and mathematicians have computed gradient and curvature profiles across certain theoretical structures, such as faults, buried spheres and cylinders, anticlines and synclines. The interpreter compares his profiles with these models and often derives

important clues; he may learn, for example, that a gravity minimum in the Gulf Coast indicates the presence of a salt dome. Remember now, that we have no more than we had before, namely a gradient and curvature map and an isogam map; the salt dome supposition came from experience which was based on drilling. Many of you have probably tried these long years to forget the consternation which was caused when gravity minima were first observed in the Gulf Coast; they were, to say the least, "unorthodox," following a number of maxima which had almost established a position of exclusiveness. It does not seem preposterous to me to assume that physicists would also have become convinced of the geologic significance of gravity minima in the Gulf Coast after the drill had established what they came from, if they would not indeed have deduced their meaning by theoretical considerations in advance of drilling.

Passing now to seismic exploration and first to refraction shooting, we recall that travel-times were observed and later plotted to yield a travel-time curve. Velocities were read from this curve, and if they exceeded about 15,000 feet per second in the Gulf Coast it was assumed that salt was present. Of course, such velocities can be obtained by shooting updip in shale, thus simulating a salt dome where none is present; on the other hand, shooting downdip in salt may give a lower, or shale, velocity. It was the physicist who found this out, because he had a background in mathematics. After a shot across Blue Ridge had indicated a high velocity, it became the vogue to lease, and to drill, high-velocity areas, based on the assumption that the velocity of an elastic wave is higher in consolidated rocks than in the surrounding sediments; this, however, had been known before from measurements made in 1888 by Fouqué and Levy in France and from earthquake seismology. *High-velocity* areas were leased and drilled, not salt domes.

In reflection shooting, it is possible and customary to observe, not only the distance and the travel-time, but also the angles of emergence of the waves arriving at the receiving stations. The angles of emergence are related to the Δt'*s* or "step-outs" on the record. Assuming a value for the depth of the reflecting layer and an average velocity, it is possible to calculate the dip from these assumed quantities, and from the travel-times and "step-outs." It should be remembered, however, that the relations used in the computations are usually derived on the assumption that only one layer is present, or that the velocity varies in the assumed manner with the depth; the

calculated value of the dip is, therefore, not to be taken at face value, but it is probably closely related to the true dip. Depths are calculated from the assumed average velocity, the travel-time, and from the shotpoint-receiver distance. Unless the average velocity was measured in a borehole in the area, the calculated depths are again only qualitative. When sand conditions like at Smackover are encountered, there must be at least two such borehole measurements, one on, and one off, the structure. Geophysicists sometimes wonder why the reflection travel-times must be converted into fictitious depths, especially in those areas where the recorded reflections penetrated several thousand feet deeper than the bottom of the deepest hole in the area, and it is consequently impossible to obtain a velocity-depth curve or to correlate the reflecting layers with geologic markers.

At this stage we wish to inquire again, what are the two geologists, the one in the field and his brother in the office, to do with these travel-times and "step-outs"? What can be done besides plotting Δt'*s* and drawing iso-time lines or plotting calculated dips and depths on a map and contouring? This procedure was precisely the one which led to the postulation of faulting by geophysicists in Gulf Coast areas at a time when the geologists were loath to recognize the existence of faults. Of course, in this rigorous use of travel-times and "step-outs" in reflection shooting and in the equally rigorous use of other geophysical data is where the rub comes. The geologist does not like the geophysicist's iso-time lines; they exhibit sharp edges, scallops and other unorthodox frills. The geologist can take the geophysicist's iso-time map and contour it so as to make it look like the "real thing." The difference resides in the fact that the geophysicist draws lines based on the physical properties which are known to vary abruptly, while the geologist thinks that he is contouring on a geologic formation which he knows to be rather well smoothed out. It should be remembered that the geophysicist's isogams, iso-time lines, etc., are not contours in the sense in which the geologist uses the word, referring to the elevation of one given point with respect to another, on the same surface; the geophysicist's iso-lines denote the constancy of a physical property along the line on the surface. To be more definite, if at a certain point the reflection time is two seconds and the "step-out" 0.030 seconds, what can you do about it? If the indicated dip and the calculated depth appear to be inconsistent with the computed dip and depth observed at a station half a mile away, what can you do about that? You can plot the calculated dips and depths or,

better, the observed travel-times and "step-outs" and contour rigorously, remembering that a geophysicist's contour may cross! On the other hand, you can follow the customary procedure and use those points which look right while rejecting the others. Let me ask you, however, if you knew that much about the area, why did you shoot it?

It appears to me that all users of geophysical methods ought to insist that:

1. Geophysical work be done carefully.

2. Data be as clear and definite as can be obtained by scientifically sound methods and apparatus.

3. Interpretations and calculations be made from the data alone and without the aid of geological information or data derived from other geophysical methods.

4. Data obtained by different methods be worked independently and interpretations plotted on different maps.

5. All geological information about the area covered be withheld from the field computers or other persons making original interpretations of geophysical data so that they can maintain an independent and unbiased point of view.

6. The maps prepared by the geophysicists be finally submitted to the geological department for study; a competent geologist, who has a thorough understanding of the physical background, can combine geological and geophysical information in the preparation of a *new* map which may then be assumed to represent all that has been learned about the area in question at that time. However, if the *new* map differs radically and essentially from the geophysical map, it must be checked by the geophysicist to find out if any of the changes are incompatible with the data. Let the geophysicist interpret and leave hypothesizing to the geologist.

7. Under no circumstances may geological information ever be used to express features on a geophysical map which are not clearly and readily deducible from the geophysical data; each geophysical map must show what was computed and deduced from the data obtained by one, and only one, geophysical method.

On the organization of an effective exploration department, I refer you to the address of our past president, Dr. Weatherby, to the last annual meeting. This paper was published in *Geophysics*, June, 1936, pp. 179–188.

Only by working as suggested is it possible to obtain independent

geophysical information which can be combined afterward by competent geologists with what is known of the geology of the area. To furnish geophysicists with geological information, to permit geologists to influence geophysical computers and interpreters, to judge the merit of a geophysical investigation by the close agreement of computed depth values with borehole data which were made available in the course of the interpretation to the computers, is to assume that the geological knowledge of the area was complete before the geophysical work was done; but *then* there was no need for making geophysical measurements.

The assumption, by the geological profession, that geophysics is only another geologic tool, has given rise to the requirement, by the geologists, that geophysics furnish an *acceptable* picture. Now let us further examine what this means. First we must have a definition of the word *acceptable* as here employed. It means, and I think all of you will agree with me, a picture in conformance with geology as it is known at the time when the picture is made. Please let me inquire, does this requirement permit of the extension of geologic knowledge except by drilling? Second, is it reasonable to postulate in advance what the measurements of physical properties must yield? Third, must the data of physical measurements which in themselves are not under our control, be so distorted as to yield a picture in conformity with geologic knowledge even to the extent of rejecting, without proper reasons, some of the data, of placing undue importance on others, all in the interest of producing an acceptable picture? The literature abounds with references to cases where drilling changed what had hitherto been an acceptable picture; when the geologic conditions became known the previously acceptable picture was no longer acceptable and the new picture which had been violently rejected before in turn became acceptable. Moreover, this must be so, for geology is not a closed book; it grows as more data come to hand. Why, then, is it so difficult to convince geologists that some of their most precious tenets or assumptions regarding the geology of any given area may, after all, not be based on proper and sufficient data?

Let us forever drop the requirement that geophysics must furnish a geologically acceptable picture. Let the geophysicist make his measurements of physical properties, let him draw maps based on the spatial variations of those properties, but let us refrain from correlating these measurements with geologic conditions. Further, let us

refrain from assuming that any apparent correlation in any one area proves the point to the extent that the same correlation must hold everywhere. It must be admitted that, not infrequently, geophysical methods do not yield geologic data *per se*.

Nor is it to be expected that the geophysical methods of the future, or those being developed at the present time, will yield such data as will be immediately amenable to the composition of geologically *acceptable* maps or pictures; and the requirement that the data of such methods be translatable into geologic language will retard, if not prohibit, progress in geophysical exploration. Each new method will be predicated for its success upon a new field procedure and the data will be different, requiring for their elucidation theoretical study (which in geophysics is always exceedingly difficult on account of the great variety of unknowns) and experience in practice. We are reminded of General Grant who, failing to appreciate the effect, on tactics, of the breech-loading rifle, persisted in the use of the mass assault, and of the generals of the World War who, having failed to study the Civil War, used the mass assault of Napoleon against machine guns with disastrous results to their troops. Analogously, we are inclined to use geophysical prospecting methods in attacking problems which are radically different from those which in other times and in other areas were solved successfully. More particularly, the refraction profiling attack of the years 1929 and 1930 in the Gulf Coast was futile; nor is it likely that the present mass assault by the reflection method in the same area will henceforth be in a greater degree economically successful. The methods remain the same, but the problems to be solved change. The success of refraction shooting in the search for shallow domes did not guarantee an equal measure of success in prospecting for deep domes; the location of deep domes is an entirely different problem. Reflection shooting solved this problem in those areas where relatively steep dips and sufficient relief were encountered.

Lately, in the search for faulted structures with small relief which may, or may not, overlie very deep domes, the problem has changed again. Strenuous efforts have been made to adapt reflection shooting to fault-finding, with indifferent success. A new approach is needed, and electric methods seem to constitute an adequate tool. I predict that in the Gulf Coast we shall soon concentrate on the location of hidden faults in the search for new oil fields. There are indications at this time that great depth penetration will not be required, and that

the delineation of regional faults which reflection shooting missed will point the way to a new era of oil finding.

In the search for structural anomalies the geologists who have been directing geophysical prospecting have forgotten to pay attention to the problem of finding stratigraphic accumulations. I refer you to the paper of the past president of the Association which he read at the last annual meeting in Tulsa. The geophysicists were cognizant of this problem, although they received little encouragement. The conception of inventions in electrical methods and in soil gas analysis prospecting dates back more than five years. Important progress has been made, and the prediction seems warranted that geophysics will in the near future be able to find oil directly and will furnish dependable methods and apparatus for finding, and locating, stratigraphic occurrences.

What then, is the proper place of geophysics? It is an independent branch of applied physics. It yields data which the intelligent geologist can use with telling advantage in his study of geology. It can give geologists important clues regarding geologic structure, and it can tell them where the geologic assumptions require modification.

Finally, what is the place of geology in Exploration? In the geophysical-geological ecotone we are already able to discern the general drift with sufficient clearness to venture predictions. It begins to become apparent that our old friend, the much belittled surface geologist, surrendered to geophysics because he did not realize his proper function in the exploration activity with sufficient clarity to withstand the onslaught. Only a few weeks ago, in connection with a historical study pertinent to a much discussed patent suit, I had the opportunity to examine the 1922–26 geologic and lease maps of the old Marland Oil Company of Texas. It was breathtaking to see that a dozen or more of our recently discovered oil fields were outlined and adequately and expertly leased more than eleven years ago. It is the irony of fate that this, the most progressive oil company at the time, first introduced geophysics into the Gulf Coast for the express purpose of better delineating these prospects already under lease, and that geophysics in less than two years shot those future oil fields out from under the Marland Company by condemning each and every one of them. Such geologists are needed today to work in advance of the geophysical methods. The most important function of the geologist in the exploration department of the future will be advance library reconnaissance, telling geophysics where prospecting will most likely

yield quick results, where from the macroscropic viewpoint sand conditions may be expected to be favorable. We are hopeful, of course, that this geologist, our guiding light, may remember when on the verge of waxing dogmatic that oil has been found in areas where geologists formerly did not permit its occurrence on account of the supposed lamentably poor sand conditions and the assumed absence of source beds; we hope also that there may be no recurrence of the blunders of those first two years of seismic prospecting.

A few aggressive geologists have developed a new field which is destined to become important; I refer to production geology. The successes already achieved in some of our recently discovered fields, in Conroe, Tomball, Dickinson, in the face of the difficulties presented by the latter two fields, give promise of greater and greater accomplishments. Already we have seen a well completed in eighteen inches of sand. Drilling speeds have increased phenomenally, an East Texas well having been drilled in thirteen days. Oil is being produced from 9,000 and 10,000 foot sands. It is true that part of this progress is due to improvements in drilling equipment, but much of it has resulted from the competent direction by the field geologist who, broadmindedly and to his decided advantage, used paleontology and electrical logging in his work in addition to his own geologic knowledge.

The geophysical-geologic ecotone will advance with geophysics finally, in the not too distant future, occupying the exploration field between the primary geologic reconnaissance work and the drill. Thus the geologist will be first, and last, in the exploration and production program. It remains to inquire what the geophysicist is doing to prepare himself for his broadened activity. He is studying geology! The geophysical exploration companies are adding geologists, not to work with geophysical field parties, but in a true staff capacity. The colleges of the land are gradually insisting that geology students study more physics, chemistry and mathematics; it behooves the older members of the profession, whether they intend to assume the "first and last" positions outlined above or not makes little difference, to become better acquainted with the physical principles underlying the geophysical prospecting methods and with their mathematical elucidation, but above all, to learn to recognize the import of physical data and the criteria which must be applied in their rejection.

The geophysic-geologic ecotone originated when the surface geological methods were no longer able to replenish the country's oil reserves. The training of the geologist definitely fits him to see indica-

tions on the surface, to postulate the probable presence of oil in a province, and in both capacities he easily excels the person not so trained, for example, the geophysicist. But the latter takes advantage of progress in the physical sciences and is enabled to see below the surface, not the geologic markers which the geologist wishes to observe, but significant variations in physical properties. This is the foundation on which geophysical prospecting is built and this has resulted in the ecotone. Finally, however, in the drilling of geophysical prospects, the geologist again enters his own element. Here again he deals with those problems which his training has enabled him to solve. He drills into the Frio and the Vicksburg; his concern is the occurrence of Heterostegina and Marginulina fossils, and the correlation of geologic markers across the oil field and from one field to the next. He can give you the top of the Frio and tell you that it is 300 feet higher here than there, remarking also, perhaps, that reflection shooting made it 600 feet; he forgets that reflection shooting delineated a velocity discontinuity which may at times be more significant physically in the middle of a geologic formation than on top of it or on the bottom.

Let us hope that, in the future, geophysicists may continue to receive the sympathetic cooperation of the geologists and that the geophysicists, in turn, may remember that they, also, have made mistakes. Let us hope, finally, that after the settlement of the controversy between the Association and the Society the two bodies may, in their new bond of affiliation, work together harmoniously for the advancement of the two sciences, exploration geophysics and petroleum geology.

DISCUSSION

ALEXANDER DEUSSEN: The paper by Dr. Blau is a careful and studied exposition of the relation of the geologist and geophysicist.

This relationship has been discussed carefully by several authors in papers published during the last few years—notably by Dr. Weatherby, presenting the case for the geophysicist, and more lately by O. L. Brace (in the February number of the Bulletin of the American Association of Petroleum Geologists), presenting the case for the geologist.

With my background of geologic training I am of course sympathetic to the view that it is necessary for close cooperation between the geologist and geophysicist in order to arrive at results both are attempting to reach—namely the delineation of subsurface structures favorable for accumulations of oil.

There is therefore little that I can add to the subject as presented by Dr. Weatherby and Mr. Brace, and in the present paper by Dr. Blau.

However, a few observations may be pertinent in this connection.

Dr. Blau states that geophysicists postulated faulting in the Gulf Coast sediments long before faults were observed and reaped the mild ridicule of geologists who claimed that "these sediments don't break, they bend."

Correctness of this statement is open to question—most of the early day geologists concerned with Gulf Coast geology inferred the presence of faults in the Gulf Coast, and according to my recollection they have always been considered as potential traps for oil.

Both Kennedy and Harris in the early writings on Gulf Coast salt domes speculated on the presence of faults in the Gulf Coast, and the map published by Harris in his paper on "Oil and Gas in Louisiana," Bulletin No. 429, United States Geological Survey, 1910, opposite page 6, has marked on it a number of hypothetical fault lines.

The difficulty that the early day geologist had was that with poor means of correlation available to him it was not possible to prove the presence of a fault, and of course the chances of finding an exposure at the surface in the Gulf Coast area was practically nil.

I am sure that the geophysicist is not entitled to claim credit for the recognition of the importance of fault structures in the Gulf Coast.

I have the feeling that if we had never introduced geophysics into the Gulf Coast that with the improved methods of subsurface correlation the rôle of faults would have been recognized nevertheless.

Dr. Blau likewise states that a geophysicist's contours may cross. I can hardly agree with the correctness of this statement, because I should think that this would be impossible. Dr. Blau certainly cannot be referring to a contour, but must be referring to something else.

Certainly the use of the word contour in this connection is open to objection.

Dr. Blau states that the geophysicist ought not to have the benefit of any geological information. I might state in this connection merely for the purpose of the record that this attitude is diametrically opposed to that which the first geophysicists assumed.

I had the privilege of having in my charge and direction the first seismograph party to operate in the Gulf Coast, this being a Minthrop crew.

One of the first difficulties I ran into with this crew was their desire to be given the benefit of all geological information that I had at my command in the areas where I had directed the shooting to be done.

I purposely refused to do this, taking the same view in this connection that Dr. Blau now takes. I wished them to determine through geophysical investigation independent of any geologic information what the subsurface structure might be.

I am in accord with Dr. Blau's views on this point, that it is probably better that the geophysicist should not have any pre-conceived notions of what the geologic structure may actually be in an area which he is investigating.

O. L. Brace: In the foregoing paper by Dr. Blau and in a discussion of my paper "Interrelationship of Geology and Geophysics" by Dr. E. E. Rosaire, written for publication in "Geophysics," the main criticism of my expressed viewpoint lies in what these writers term the *orthodox* attitude of the geologist toward structural interpretation, which results in his insistence upon a picture that is *acceptable*. This attempt to place the toga of orthodoxy solely across the shoulders of the geologist represents a distortion of the facts. In an early part of his paper, Dr. Blau states that the geologist

should "call to mind the prospects that were missed because pertinent data were disregarded," yet at a later point he relates, with tragic emphasis, how Marland Oil Company of Texas outlined, at an early date, by means of applied geology, a dozen or more structural areas which have subsequently been brought in as producing fields and how "geophysics in less than two years shot out . . . and condemned each and every one of them." This is obviously a case of orthodoxy, in geophysics, at its worst.

It is true that the geologist is too frequently, in his interpretation of structure, a victim of a restricted background and, particularily where he has been forced to concentrate within a given province over a period of years, is there a tendency to draw interpretative conclusions that are colored by the influence of a given structural type. A drift toward orthodoxy, however, is a weakness that plagues all science and it little behooves the geophysicist to call the kettle black. As a matter of fact, the orthodox attitude of the geophysicist today is one of the greatest handicaps to the successful continuation of reflection seismic work. Both Dr. Blau and Dr. Rosaire forecast, with regrets, the early passing of the reflection seismograph as the outstanding exploratory method, all because of the shortage of first class domes. The second and third class prospects, of which there are a great number at the present time, are of this lower classification because they do not fit the requirements of the orthodox number one Iowa type. Yet, unquestionably, many of them represent favorable structural traps and, with proper interpretation, may lead to the discovery of oil reserves greater than those represented by the more spectacular first class domes of the past. A case in point is Tepetate. At the first mapping of this structure, the reflection seismologist was quick to give it an unfavorable rating because it did not conform to the more desirable major-closure type of structure.

Speaking commercially, both the geologist and the geophysicist are concerned with one problem: the location and delimitation of structural traps favorable to the accumulation of oil and gas. Quite evidently, each has his own place in the solution of this problem. It has not been my intention to advocate that the geologist should concern himself with those results of geophysical exploration that are of a purely abstract nature. If the geophysicist cares to contour with "sharp edges, scallops and other unorthodox frills" in an effort to present "measurements of physical properties . . . that are not to be correlated with geological conditions," that is surely his privilege and he has the right to resent the interference of the geologist in this highly specialized, and unquestionably intriguing, activity. The experienced geologist, however, knows that structures are essentially smooth and regular in their configuration and he is not to be blamed if he attempts, at the last analysis, to produce a picture that, while not necessarily orthodox and acceptable, is at least natural. As long as the geophysicist is dealing with measurements of physical properties that are understandable only to the physicist, then the geologist must content himself with his tasks as librarian, as Dr. Rosaire has it, and not intrude himself with his structural theorizing. It is when the geophysicist produces records that are purported to represent direct readings of strike and dip that the geologist enters the scene as a vital necessity. Even under such circumstances, the geologist, theoretically, becomes less a factor as the number of structural control points approaches infinity but the difficulty of this approach makes interpolation a necessity and it is structural interpolation, in turn, that requires a breadth of knowledge of structural occurrence.

Both Dr. Blau and Dr. Rosaire have attempted to make a strong point of the fact that geologists, in the past and, therefore, by assumption, in the present and future,

have failed to recognize structural elements that have since prove to be important factors in oil accumulation. A case in point, according to their contention, is that of the significance of faulting along the Mexia zone and the presence of faulting on the Gulf Coast. In my paper, I made the statement that "the complexity of structural forms under which oil and gas may accumulate was not recognized until this diversity was brought to light in the natural progress of oilfield development," which is another way of saying that structural geology is a living science in which the fundamental, governing laws have been revealed directly as the result of experience. Due to the progress of the science prior to the Mexia days, I am inclined to question the statement that experienced geologists, conversant with the facts, did not recognize the significance of the faulting at Mexia and, subsequently, on the Gulf Coast.

Before closing, I want to take issue again with Dr. Rosaire in one of his statements in his discussion of my paper. He, therein, accuses the early Gulf Coastal geologist of incompetence, on the evidence that during the period from 1917 to 1931, 675 coastal wildcats were drilled and but one dome was discovered. His implication is that while the geophysicist, during the recent past, has made discoveries too numerous to mention, the geologist in the preceeding 14 years, was able to gather only sufficient geological evidence to result in one discovery. He, deliberately or otherwise, ignores the fact that a great number of these 675 tests were perfect locations on prospects that have since proven productive in deep sands and that the "lack of higher structural finding powers" which he deplores was, in reality, a lack of the means and the incentive for deep drilling. I do not exaggerate the facts in stating that the modern reflection seismologist, of whom Dr. Rosaire is a most successful exponent, came into being with a proverbial silver spoon in his mouth. Prospects were laid out for his selection like pies on a shelf. Fifteen years of intensive study by the surface geologist, followed by the pioneer gravity man and the refraction seismologist, had isolated a vast number of favorable prospects, but these remained untested because of the slow advancement of deep drilling technique and, in the latter part of the period, of a glutted oil market and a generally depressed condition throughout the oil industry. In 1931 and 1932, the reflection seismologist, emerging with a new method of exploration, was met with a favorable change in all of those retarding factors of the past, and in a period of three or four years, he claimed and carried off many of the trophies that had accumulated over a period of 15 years. Obviously then, the early coastal geologist, rather than being incompetent, had reached a high state of professional efficiency. This is further attested by Dr. Blau's recital of the tragic experiences of the Marland Oil Company of Texas during this same period.

There is not space here to analyze Dr. Blau's paper, point by point. In my original paper, I had no desire to offer cause for friction between the two branches of the exploratory group, but rather to clarify a relationship that seems unnecessarily confused. In the present discussion, therefore, I have taken issue mainly with the accusation that the geologist, alone, is a victim of orthodoxy and I here contend that he incorporates this weakness in his scientific viewpoint to no greater degree than does the geophysicist or the practitioner in any other active science. Since, in reference to structural interpretation, the term *acceptable* has proven so controversial, I would substitute the word *natural*. Let the geophysicist give free reign to his imagination with his frills and crossed contours but give the geologist, also, the opportunity to express his interpretation of geophysical structures as nature designed them: smooth, simplified and without undue minor complexity.

D. C. BARTON: The question of the place of the geologist and of geological knowledge in the interpretation of geophysical data has started many a vigorous but friendly dispute between Dr. Blau and myself. I quite sympathize with his reaction to the fact that some geophysicists have worked their geophysical interpretation backward to get an answer congruent with some geologist's conception of what the answer should be. I heartily agree with the high advisability of making the first analysis unbiassed by preconceived ideas from geology or from any other geophysical method and as unbiassed as possible by subjective working over of the data; and I heartily agree with the high advisability of keeping a record, commonly it will be a map, of this first analysis, so that the geophysicist himself or anyone else who wishes to check or re-work the interpretation can go back of all subjective bias. But, if the geophysicist can not make a complete solution from his particular type and batch of data, I believe that he should then consider all other lines of evidence that may give him a clue to the interpretation of his data. The anomaly of a geologic feature commonly may be recognizable in the geophysical picture only if the geophysicist knows of the presence of the structure and knows where to look for the anomaly. In checking prospects, often it is important to work back that way and find out whether the particular batch of geophysical data can be interpreted within reason as indicating or suggesting corroboration or modified corroboration of the structure surmised for some other reason, but the geophysicist should keep carefully in mind what he has done and, in reporting his conclusions, he should express the dependence that should be placed on them. Geology and the geophysical methods of exploration are inexact sciences, and almost every interpretation by either a geologist or geophysicist of what is underground is accompanied by a host of "ifs," "ands" and "ors" in the geologist's or the geophysicist's mind. It is very difficult for a geologist to explain them all except to another geologist, or for a geophysicist to explain them except to another geophysicist. In spite of my many arguments with Dr. Blau, I still wish to maintain a) that the best geological interpretation of geophysical data will be obtained by using a man who at once is an able geologist and a geophysicist, and who at one and the same time can evaluate and compare the geological and the geophysical "ifs" and "ands," and b) that the content of special mathematical-physical and seismic theory now available and usable for the geologic interpretation of seismic surveys, or the content of comparable knowledge now usable in geologic interpretation of gravitational and magnetic anomalies is not too large or too complicated to be taught to a well trained geologist. I feel quite sure that the content of that knowledge in any one of those three geophysical methods is considerably less than the content of physics, mathematics, and special knowledge that the advanced petrographer has to have.

In spite of my agreement with part of Dr. Blau's thought in this paper, my dissent is profound in regard to many of his statements of fact, to much of his reasoning, and to many of his conclusions. But I shall particularize my dissent only by showing the fallaciousness of one of his arguments in which he virtually is formulating a tenet of faith that rapidly is becoming orthodox among Texas geophysicists. The statement is made (Page 96) that "Thus geophysicists postulated faulting in the Gulf Coast sediments long before faults were observed and reaped the mild ridicule of geologists who claimed that 'these sediments don't break, they bend' . . . Geophysicists are thus less reluctant to produce 'unorthodox' pictures . . . To the extent that the geologist is guided entirely in the interpretation of his data by what is known or believed to be possible, to that extent he must rely upon the geophysicist to cover such 'unorthodox' occurrences." The fallaciousness of that argument is quite evident to any one who

knows the history of geologic knowledge and thought in regard to the Gulf Coast.

The geologic postulation and knowledge of faulting in the Gulf Coast has had a long and honorable history:

1903 Hayes and Kennedy (U. S. Geol. Sur. Bull. 212) referred "the origin of the salt domes to supersaturated solutions coming through fissures opened along a fault line" and postulated "movement along these lines including both flexing and faulting as far back as Miocene into the present."

1907 Harris in his "Salt Report" wrote, "The longer we study these peculiar structures the more convinced are we that, although they may be located along lines of weakness, faults . . . "

Hayes, Kennedy, and Harris, of course, knew only the very shallow domes and were thinking only of the upper few thousand feet of beds.

1913 Lee Hager anonymously in "Indication of Oil in the Gulf Coast Country" (P. 11) speaks of faults and faulting but it is not quite clear whether he is thinking of faulting involving the beds within reach of the drill.

1914 A. Deussen, U. S. Geol. Survey Paper 335, (P. 86): "Additional lines of flexing and faulting in the Coastal Plain are inferred from the deposits of oil, salt, and gas, and from evidence of fossils."

1924 A. Deussen, U. S. Geol. Surv. Prof. Paper 126, (P. 124): "Faulting is not conspicuous in the Tertiary and Quaternary formations of the Coastal Plain east of the Balcones (and Mexia-Luling fault) zone, but a few faults have been noted in these formations." (P. 126): "Finally the fact that folds and faults mark the boundaries of this (Tonecillas) uplift . . . , " (P. 128): "Faults may accompany the anticline (Whites Point) . . . , " (P. 129): "Another line of folds and faults, known as the Senton anticline . . . , " (P. 129): "Another hypothetical zone of folds and faults called the Rockport anticline . . . "

1924 In "Geology of Salt Dome Oilfields":

George Bevier, (P. 631) wrote in regard to Damon Mound, "Faulting is present generally in the form of step faults extending along lines parallel to the circumference of the dome. . . . Radial faulting has been noted."

A. E. Minor (P. 474, Fig. 3) shows faulting at 2300–3200 feet in the Edgerly oilfield. The type of the production at Edgerly in many ways shows closer affinity to the production on the very deep domes than to the shallow domes.

F. E. Vaughn (Pp. 381–383) records faults in the sea cliff at Côte Blanche.

George Bevier (P. 537) "Circumferential step faults . . . are proved and radial faulting has been noted . . . The presence of faulting can be established at Barbers Hill . . . "

D. C. Barton (P. 175) in discussion of the American salt dome problems wrote "Evidence that faulting . . . could initiate an upward current of water . . . through many thousand feet of gumbo and sand."

1925–26 The German geologist-geophysicist who did the interpretation for Seismos were prolific postulators of faults at depths less than 3500 feet in the Gulf Coast (I do not know which side of the argument is favored by this fact, but, nevertheless, these geophysicists were primarily geologists, and were able geologists and had utterly no reluctance in postulating faults in the unconsolidated sediments of the Gulf Coast).

1929 In "Structure of Typical American Oilfields": P. W. McFarland in "Laredo District, Texas" wrote (I. P. 396), "Faulting and sand lensing are the principal

factors in the accumulation of oil and gas in this district," and in Figs. 2 to 6, he figures major faults. Most of the oilfields of the Laredo district, of course, are on the southern equivalent of the Conroe trend.

D. P. Carlton in "West Columbia Salt Dome Texas" (II. P. 451) figures and discusses several major faults in the flank beds.

1931 (Jan.) D. C. Barton in "Effect of Salt Domes on Accumulation of Petroleum." (P. 61) "Accessory peripheral and radial faults are common on salt domes." (Pp. 65–66) "Similar accumulation (of oil) may be found on normal faults with down throw on the down-dip side, which cut soft unconsolidated sediments and which are not necessarily associated with salt domes. The trap controlling the accumulation of oil in the Miocene sands at Orange is surmised by the writer to be of this type."

In connection with the high reluctance of the geologist to do anything unorthodox, it might be interesting to call to mind the geologists: DeGolyer, who in 1913 not knowing of geophysical exploration, came to the conclusion that geophysical exploration must be the next tool tried by the geologist and who pioneered so actively in its exploration; David White, who in the early 1920's induced the U. S. Coast and Geodetic Survey to take pendulum observations primarily for a study of geologic structure and who in 1924 gave his presidential address on "Gravity Observations from the standpoint of Local Geology"; Van der Gracht and Ganett who respectively, in 1923 and 1924 so warmly received the geophysical methods and pioneered in their application, and other of us geologists who went over into geophysics in 1922–1925 before most of the members of the S.E.G. had heard of geophysics.

RECENT DEVELOPMENTS IN EXPLOSIVES FOR SEISMOGRAPH PROSPECTING*

W. R. FARREN† AND H. H. WHITE‡

In the early days of prospecting with the seismograph in this country, more attention was devoted to the development of instruments than to the explosives which provided the impulses they recorded.

It was difficult for the manufacturer of explosives to cooperate with the prospectors toward providing a type of dynamite which would be best suited to their requirements without knowing the type of impulse desired, and the conditions under which the explosive was to be used in the field. For some little time, this information was not obtainable because geophysical methods using the seismograph were confidential and visitors to the parties in the field were discouraged.

Wide Variety of Explosives used in Early Explorations: During this early period, demands from prospecting crews covered the entire range of regular commercial explosives from black blasting powder, which deflagrates at a very low velocity, to the high percentage nitroglycerine types of dynamite which detonate at extremely high velocities. The action of this wide range of explosives would provide shock effects upon the earth ranging from a cumulative push to a sharp, almost instantaneously applied shock of great violence.

Then, too, the conditions surrounding the use of the various types in the field would greatly affect their efficiency, since some of them are not designed to withstand moisture.

With the steady and rapid improvement of instruments and technique, it became apparent that haphazard selection of a commercial explosive was both impractical and wasteful, and some effort was made toward selection of an explosive from the large variety of types then available, which could be depended upon to give the uniformity of results essential to the success of the seismographic method of prospecting.

Numerous field tests were conducted in which representatives of explosive manufacturers were invited to cooperate. From their knowl-

* Paper read at the Fall Meeting, Houston, Texas, November, 1936.

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edge of the behavior and characteristics of their products under conditions thus found to exist in the field, the selection of possible types of dynamite was narrowed considerably.

General Adoption of 60% Ammonium Nitrate Gelatin: The records obtained from these tests were the deciding factor which brought the almost universal adoption of 60% Ammonium Nitrate Gelatin for this class of work.

The adaptability of this 60% Gelatin was undoubtedly due to its inherent physical and explosive characteristics, the important features being adequate strength, good water resistance and high velocity when shot under confinement.

In addition, the formula is sufficiently flexible to permit manufacturing several degrees of hardness and thus it could be made suitable for practically any loading condition.

Difficulty of Obtaining Full Velocity: In respect to velocity, it should be noted that the rate attained is in a large measure dependent on the degree of confinement afforded. While it is realized that in the field, every effort is made to keep this condition uniform, yet there is no assurance that uncontrollable variations are not of sufficient magnitude to affect velocity somewhat, and thus produce conflicting records. It has been inferred that increasing confinement results in increased velocity, and this is generally correct, but only up to a certain point. At this point, which might be termed the critical pressure, velocity tends to decrease, and in extreme cases, the wave dies out entirely, resulting in incomplete detonation.

Use of Inner Core to Insure Full Velocity: In order to overcome these obvious objections and insure a uniform velocity under all conditions, two methods of attack have been adopted. The first of these, which has been available for several years, is the use of a dynamite core. This core, unlike the gelatin, is much less affected by pressure. Thus the charge, once detonation is underway, takes the rate of the type of dynamite employed. Accordingly, not only is a more uniform rate provided, but one which can be varied, if so desired, by the selection of the type and size of the core.

For such cores, the relatively highly aerated straight nitroglycerine dynamites have been extensively used, which, when protected from water by the surrounding gelatin, provides much greater reliability under high pressures.

Use of Light Carbonaceous Materials: In the second method of providing uniform velocity, the same principle has been adopted,

but in this case the aeration has been spread uniformly through the mass of gelatin by means of light carbonaceous material. One material employed at present is a finely ground cork. To the best of present knowledge, it is the most suitable combustible available, since it is unaffected by water, and its low nitroglycerine absorption tends to insure good keeping qualities.

The introduction of the low density cork together with the increase in aeration has naturally resulted in a lower density or lighter weight per cartridge for this type gelatin.

Although it is difficult to duplicate field conditions in the laboratory, in the development of this explosive, the various types possible were subjected to innumerable water tests, involving pressures equivalent to about a 500-foot head.

In addition to the selection of cork, these tests pointed the way to the most desirable method of manufacture which, in brief, consists of first preparing a highly gelatinized mass of nitroglycerine and nitrocotton into which the carbonaceous materials, oxygen carriers, etc., are actually kneaded. Since both formula and manufacturing conditions must be closely controlled, this gelatin at present, is offered in only one degree of hardness.

The water resistance, or the resistance to penetration of water of this type of explosive is dependent, to a large degree, on the gelatinous or rubber-like character attained.

Cork Reduces Wicking: However, it is important to avoid ingredients that offer a pathway for the entrance of water through wicking or continuous pathways. These factors may be considered somewhat inconsistent with the previously accepted theory that a high degree of aeration is important for high velocity. It is obvious, therefore, that the problem is one of introducing the most resistant aerating material. It will be readily admitted that cork offers the maximum in small air cells within a given volume and that these cells may be smaller under high compression, but continue to exist. It would be futile to provide air cells which collapse completely under pressure. It would be equally futile to provide aerating material which deteriorates under the influence of water and cork does not so deteriorate. Further, the small cells of cork are separated by woody diaphragms, so continuous pathways are avoided. Also, it is our experience that, among aerating materials, cork is preferable from the standpoints of wicking and penetration. In theory, the cells at the surface are covered and sealed with films of the rubbery, gelatinous, resistant product of nitrogly-

cerine and nitrocellulose. Smaller cells support the films against rupture more effectively than larger cells; also, the known resistance of the cork body offers sturdy support to the films as regards rupturing through strains. Numerous tests seem to support this theory.

Since the degree of penetration is largely dependent on the period of exposure, the time factor as well as the pressure involved must be taken into consideration when devising a test to enable the successful prediction of field execution.

Water Resistance Tests: This has been proven by tests which show non-penetrated formulae to evidence no reduction in sensitiveness and velocity after being subjected to 200 pounds water pressure (equiva-

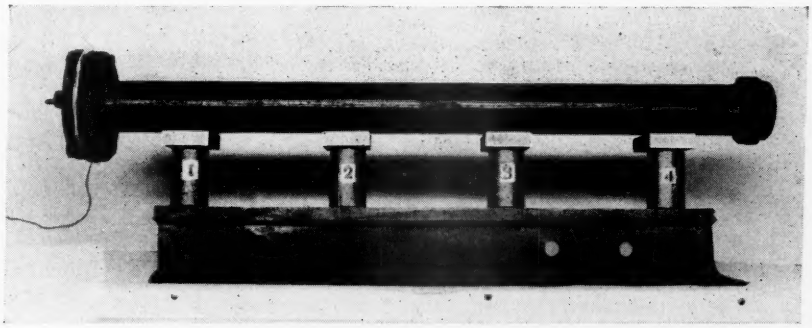

FIG. 1

lent to a 465-ft. head) for five hours, whereas with definitely penetrated types, reductions in sensitiveness of 20 inches (28 inches to 8 inches) and decrease in velocity as high as 5,000 ft. per second (15,000 to 10,000 ft.) have been experienced.

Tests of this nature have the defect from the practical viewpoint, that the final shooting is completed out of contact with the water, after the pressure has been removed.

In order to maintain the powder both under water and pressure and thus more nearly duplicate field conditions, some sort of a bomb type assembly is required. A paper has been presented before this body covering a very novel test of this nature.

Briefly, the assembly consists of a piece of 3-inch pipe closed at one end with a standard pipe cap, and at the other end with flanges equipped with rubber gaskets which permit the entrance of the blasting cap lead wires (Fig. 1). The charge consists of three 2″×16″

cartridges. The pipe is filled with water and maintained at the pressure desired for specified time intervals.

In the actual testing of the powder, the efficiency is judged by the effect of the explosion on four lead cylinders which, for the sake of rigidity, are placed on a steel rail. The cylinders are protected from flying fragments by small steel plates.

If, after conducting a test, all of the cylinders show an equal amount of compression, it is concluded that the explosive wave has

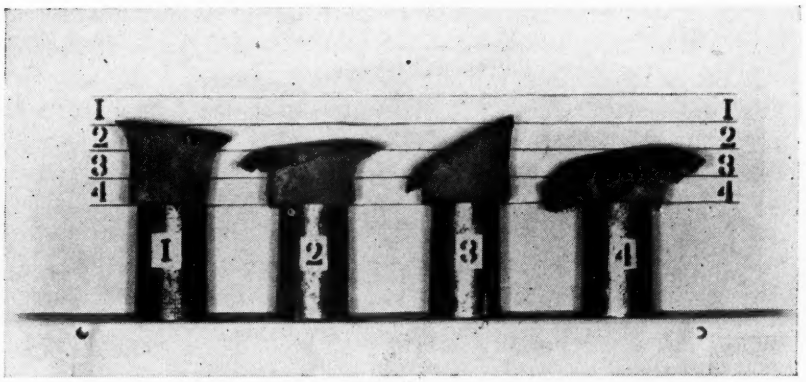

FIG. 2

proceded at a uniform rate throughout the entire length of the column and thus the powder under consideration should exhibit the desired behavior in seismic prospecting.

Fig. 1 shows the bomb assembly in position, and Fig. 2 the results of a typical test on a 60% High Velocity Type Gelatin.

The cylinders designated as 1, 2, 3 and 4 in Fig. 1 are the standard size—$2\frac{1}{4}'' \times 4''$—and are included simply as a basis of comparison. Some irregularity is evidence by the crushed leads but the fact that number 4 shows a satisfactory reduction is evidence that detonation of maximum efficiency was being experienced at the end of the column.

DEVELOPMENTS OF ESSENTIAL CHARACTERISTICS IN ELECTRIC BLASTING CAPS FOR SEISMOGRAPH PROSPECTING*

G. F. ROLLAND† AND H. H. WHITE‡

The exacting requirements of seismograph prospecting call for two essential characteristics in electric blasting caps; *uniformity* and

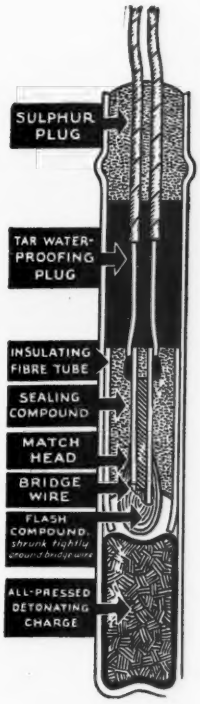

FIG. 1

safety. The degree of uniformity demanded has required new standards in the manufacture of electric blasting caps, and a great deal of research has been carried on to this end.

* Paper read at the Fall Meeting, Houston, Texas, November, 1937.

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ASSEMBLY OF THE MATCH HEAD

The heart of the seismograph cap is the electric match head. Functioning characteristics of the cap, in a large measure, are dependent upon it. In its production very accurate machines are employed. Component metal and insulating parts going into the formation of the match assembly are required to conform to strict specifications with variation limited to the thousandth of an inch.

BRIDGE WIRES UNIFORM IN LENGTH AND TENSION

The length of the electric match head step across which the bridge wire is stretched, is governed by micrometer adjustment so that the length of the bridge wires is constant, a primary factor tending to give uniformity in firing characteristics.

The bridge wire used in seismograph caps is made of a carefully selected alloy. It is highly resistant to corrosion and of great tensile strength so that it may be stretched tightly across the step without distortion. The diameter of the bridge wire is closely controlled within limits of .0012″ to .0014″ to maintain uniform resistance per unit length of bridge.

After the rigid match base assembly has been prepared, the bridge wire is spanned across the gap and held in place under tension until fixed in position with soldered joints. Excess bridge wire is then carefully removed. From a description of these operations it can be realized that because the length of step is uniform and the wire is stretched across under tension, the resistances of the bridges are, for all practical purposes, identical.

FLASH POWDER TIGHTLY SHRUNK TO BRIDGE WIRE

Maximum efficiency in heat transfer from bridge wire to flash powder is obtained by:

1. Precipitating the powder in almost colloidal form. This produces a large surface area per unit weight that insures a maximum degree of contact with the bridge. Agglomeration of particles is prevented by unique methods in preparation. The flash powder is passed through a 120-mesh sieve just prior to use. Variations in ignition temperature of flash powder due to varying humidities during the drying period have been eliminated by air conditioning the plant.

2. Suspending the flash powder in a nitrocotton lacquer to form a paste-like compound and fully surrounding the bridge wire with it. This compound, on drying, shrinks very tightly around the bridge,

insuring efficient heat transfer. To illustrate, if you grasp a hot pipe loosely, the heat transfer is light. However, if it is grasped firmly, a severe burn is likely to be the result.

Frequent reference is made to the match head type of electric detonator and there may possibly be some misconception in that it may be thought to have slow starting properties such as exhibited by an ordinary match which, when struck, first sparkles and sizzles and finally bursts into flame. Not so with the electric match. It functions with an explosive burst of flame of such volume and intensity that it instantly initiates the entire surface of explosive charge beneath it.

INSULATING AND TESTING THE COMPLETE ASSEMBLY

The complete electric match head assembly is insulated from the copper shell of the cap by means of a fibre tube. This precludes accidental contact between the firing circuit and shell which is highly desirable from the firing standpoint as well as providing insurance against accidental firing of the cap through a ground to the shell.

Finished electric match heads are double tested before assembly into complete electric blasting caps. This test is conducted by means of a delicate Wheatstone Bridge arrangement that shows the true resistance of the bridge. Only those matches falling within a limited range of 0.10 ohm variation are used in seismograph caps. After the assembly of the caps is fully completed, and following all other types of inspection, the seismograph caps are finally double checked with the same type of delicate Wheatstone Bridge instruments in order to eliminate any caps which do not fall within the narrow limits of resistance that are maintained.

TESTING TIME—CURRENT CHARACTERISTICS OF ELECTRIC BLASTING CAPS

Studies of the time-current characteristics of electric blasting caps have been under way for some years. Data of fundamental importance have been obtained by means of the oscillograph.

OSCILLOGRAPH TESTS

In Fig. 2 the time breaks of standard and seismograph caps have been plotted. From this curve may be taken the time that is required for a current of given amperage to detonate the cap. It follows from the graph in Fig. 2 that an ample firing current should be used if a high degree of uniformity is to be obtained.

METTEGANG RECORDER TESTS

Although the oscillograph is a very helpful instrument in studying the transient phenomena which are connected with the firing of electric blasting caps, the Mettegang Recorder has also been used to good advantage, especially in detecting small time lags or differences between caps fired under identical conditions.

For studying time lag between two caps, enameled copper wires (No. 30 B&S gauge) were stretched across or wrapped around the

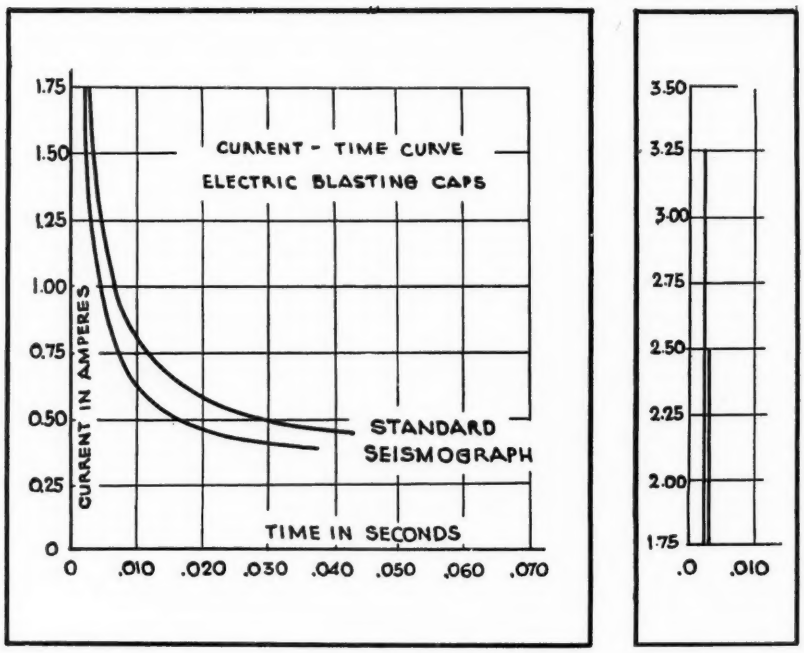

FIG. 2

bases of the caps and held in a taut condition. Explosion of the cap charges caused breaks in the primary circuits of induction coils which formed part of the circuit, throwing sparks on a smoked drum running at constant speed. By this apparatus, times of the order of one millionth of a second may be measured accurately.

A number of comparisons made by this method might be of interest. Caps with a fulminate chlorate base charge were compared with caps having a tetryl base charge. Average time difference between two caps fired through 50 ohms with a No. 3–50 Hole shunt wound blasting machine was as follows:

Caps with Fulminate Chlorate base charge.. .00007 sec.
Caps with Tetryl base charge..... .00008 sec.

The effect of decreased resistance or increased current in the firing circuit is indicated by the following comparison:

Caps tested in pairs using a total of 60 ohms resistance.... .00017 sec.
Caps tested in pairs using a total of 50 ohms resistance.... .00008 sec.

As stated previously, it is again indicative that ample firing current is a means for producing uniformity of results.

CORDEAU TESTS

A practical, yet extremely accurate test, was devised as an independent means for checking time differences. This test involved the use of 6 to 10 ft. of Cordeau detonating fuse on the ends of which the caps to be tested were securely fastened. Upon firing the caps under a given set of conditions, explosion of the TNT in the Cordeau was initiated at both ends, the explosive wave traveling at constant speed toward the middle of the fuse; and provided, both ends were initiated at the same instant, the waves met at or very close to the middle mark and registered a depression on a lead bar upon which the Cordeau was fastened. While the test was limited to differences of the order of .0002 sec., it demonstrated the effect of cap lag very vividly and checked Mettegang results reasonably well. Comparative results were as follows:

Mettegang Test	*Cordeau Test*
.000115 sec.	>.000067 sec.

EFFECT OF PRESSURE USED IN PRESSING CHARGE

During one phase of the seismographic investigation the effect of pressure used in consolidating the explosive charge was investigated. Under one particular set of conditions caps were tested in which the explosive charge had been pressed at 150 and 300 lbs. total pressure. Comparative results follow:

	Average Time Lag
Caps pressed at 150 lbs.	.000083 sec.
Caps pressed at 300 lbs.	.000164 sec.

In order to avoid any non-uniformity which might be caused by pres-

sure variation from cap to cap, a cap press has been resorted to in which the press pins operate against a constant hydraulic head. This insures uniform pressure from cap to cap and, at the same time, prevents the possibility of overpressing the charge.

TESTING TIME-LAG IN SERIES

In an effort to enlarge the scope and reliability of the tests a method for shooting 10 caps in series through 50 ohms using a 50 shot blasting machine was developed. In making this test, all caps were mounted rigidly and barricaded from each other. One set of wiring consisted of the fine break wire wrapped around each cap and connected in series. The explosion of the first cap breaks this circuit and registers a point on a smoked drum revolving at constant speed. The other set of wiring consists of fine break wires connected across each cap in parallel so that the circuit is not completely broken until the last of the 10 caps fires. When that occurs, another spot registers upon the drum and the time elapsed between the firing of the 1st and 10th cap may be determined. This test is applied as part of the specifications on all seismograph electric blasting caps. Lots which show a time lag appreciably greater than .0001 sec. are rejected.

In view of what has been said before, it should be noted that the uniformity claimed for this type of cap is based upon the match head, which involves the use of a flash paste coated bridge. Contemporary research indicates that this theory is now generally accepted, and the trend is toward this type of construction for caps which must fall within the fine limits of accuracy demanded by modern seismographic exploration practice.

SEISMOGRAPH FILAMENT TUBE

There has been considerable discussion and some complaint about the lag or non-uniformity when two caps are fired in series in seismograph work where one cap is used to explode the charge and the other to register the time-break or time of explosion on the seismograph. A study of this situation revealed that the break wires used around the cap not buried in the explosive charge were possibly not uniform and that the use of heavy wires produced especially misleading results, because the wires stretched before they broke. At that time there was developed what was known as a seismograph filament tube to be slipped over the indicating cap, but the trend away from

the use of two caps in series led to the abandonment of this idea which would have improved uniformity of results considerably.

SPECIAL SEISMOGRAPH BLASTING MACHINE

Many of the crews in the field have been disturbed occasionally by turning out records showing very indefinite time breaks. Records of this description must cause a great deal more concern to the computor when they reach him from the field. Some of these indecisive time breaks may be attributed to re-establishment of the electrical circuit in the hole after the cap has fired. The introduction of a special blasting machine which throws all its energy into the circuit over a period of .005 second has proven to be of considerable help in overcoming this difficulty.

DEVICES FOR SAFETY AND CONVENIENCE

Electric blasting caps used in seismograph work are often transported over rough country, and are subject to usage around electrical instruments where the possibility of accidental discharge by contact with stray electrical currents necessitates precautionary measures. Therefore, an endeavor has been made to develop an electric blasting cap which will safely withstand the abuses of rough transportation. And, it is believed that seismograph caps now embody safety features which insure maximum protection against accidental discharge from stray electric currents if these should be encountered.

A recent development in the assembly and packaging of electric blasting caps has been the folding of the cap leg wires accordion-wise so that the folds protect and cushion the cap on the side as well as the ends. Rough but practical tests indicated that a 10-pound cylindrical steel weight could be dropped from a 10-ft. height through a vertical 2″-pipe as a guide, on to a completed package of this kind without detonating the cap.

Another interesting and practical demonstration of the added protection of the package was afforded by purposely exploding one cap in the center of a carton of 25, which is about the same size as a shotgun shell carton. From a series of tests it was noted that, at the most, only 4 of the caps immediately in contact with the initiating cap exploded. The remaining individual packages were somewhat damaged and scattered about but the caps did not detonate.

For seismograph caps in the longer lengths the same idea has been further developed into a fold which offers ease in handling and use.

The wire is folded in sections and the removal of a simple tie permits extension without kink or snarl.

Another safety feature developed to give utmost protection against accidentally firing the cap with stray currents is the metal safety shunt. Because the safety shunt is easily removed it reduces the tendency for men to remove this safety device before making final connections.

The exacting requirements of electric blasting caps for seismograph work have been discussed and the steps involved in producing an improved product have been outlined. Various technical methods for evaluating the essential characteristics have been given in some detail. Special developments in seismograph electric blasting caps and blasting accessories tending toward greater efficiency and safety in seismographic practice have been described.

THE SPECTROGRAPHIC CORRELATION OF OIL WELL WATERS*

M. F. HASLER†

ABSTRACT

A spectrographic analysis of oil well water brines is described, which allows the detection and measurement of a much larger number of elements than the chemical method usually employed, with a considerable saving of time. This larger number of correlation variables have been analyzed statistically and found to give correlations in many cases where the usual chemical tests were inadequate. It is proposed that the same methods may be applied to core analysis.

It has been recognized for some time that an elemental analysis of the salts of oil well waters may be a powerful method of correlating waters as to their origin. With this as a basis, certain chemical analyses have been established to determine as large a group of elements as possible, consistent with economy. Usually six variables are so determined and the results plotted on a star shaped diagram. The success achieved by this method has in some cases been considerable, while in other cases complete failure has resulted.

The obvious way to improve such correlations is to increase the number of variables that can be measured in each sample. This is common practice in anything of a statistical nature. Since this must be done with due respect for economy, an entirely new analytical method must replace the chemical one.

The spectrographic method of analysis has been found particularly suited to correlation work because of its analytical speed, sensitivity, and generality. Thus instead of six variables, fifteen or so can usually be employed for correlation work. Instead of taking a longer time to get more data, the analytical time has been reduced by a factor of at least five over that of the chemical method; and furthermore, cases in which the older method could definitely not be used are successfully correlated by the new method. Added to all these advantages is the fact that a permanent photographic record is obtained of each analysis which can be rechecked should the necessity arise.

One of the reasons why spectrographic analysis is so rapid, efficient, and economically feasible in correlation work, at the present

* Paper read at the Annual Meeting, Los Angeles, March, 1937.

† Applied Research Laboratories, Los Angeles, Calif., Introduced by L. W. Blau.

time, is that new instruments have been developed expressly for that purpose.

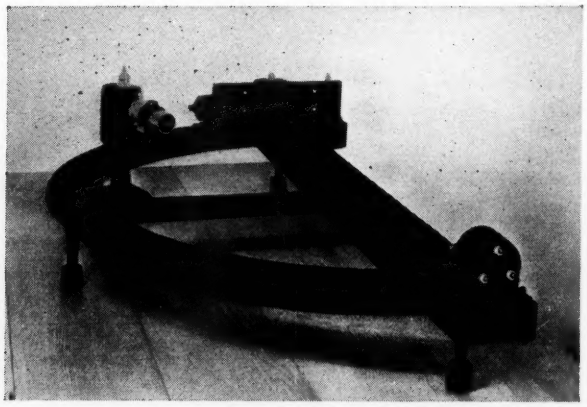

FIG. 1

Fig. 1 shows a grating spectrograph particularly suited to either water or core sample analysis. For the latter particularly, an instru-

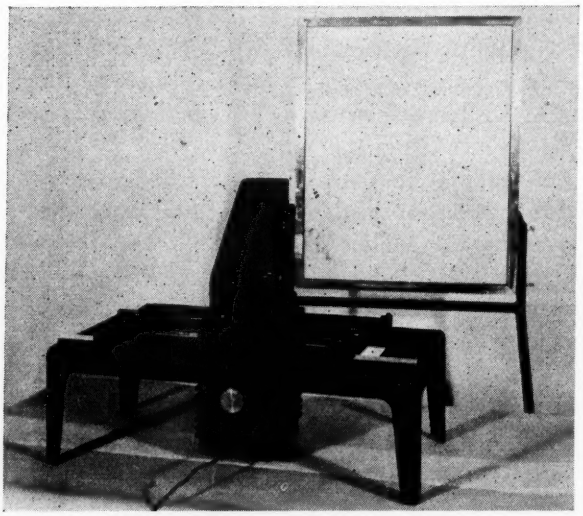

FIG. 2

ment of high dispersion and resolution is an absolute necessity. The grating spectrograph is preeminent in this respect.

Fig. 2 shows a projection comparator which allows the photographs, or spectrograms taken on the spectrograph, to be analyzed in a very short time. The positions of the lines along the film indicate the elements present, while the length of the lines read on a vertical scale indicate the quantity present.

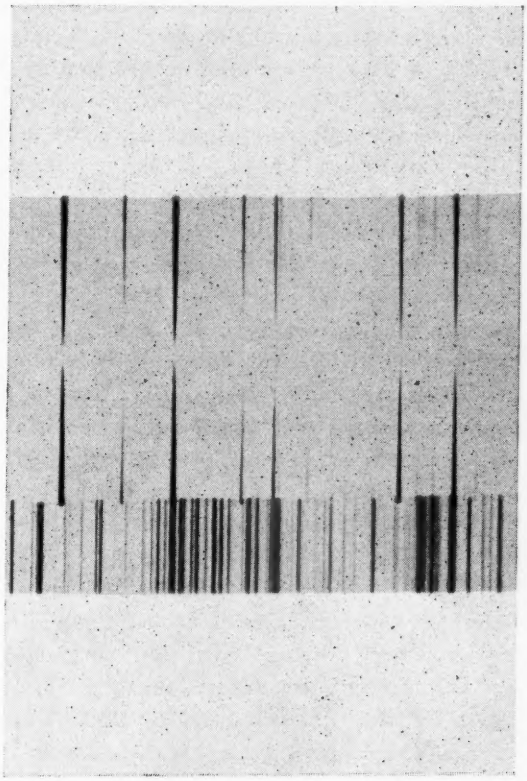

FIG. 3

Fig. 3 represents typical portion of a spectrogram and demonstrates how the lengths of lines vary with the lines' intensities.

To appreciate fully the simplicity of the method, the analytical procedure employed will be briefly discussed. Well waters may be evaporated to dryness and the salts volatilized in a direct-current, carbon arc. F. H. Emery[1] has advocated an alternative procedure in which the water is placed directly in the heated anode creater and evaporated therein, after which the salts deposited are arced. The

[1] F. H. Emery, Oil & Gas Journal, March 4, 1937.

light emitted by the discharge is photographed by the spectrograph producing a spectrogram, the permanent record of the analysis. This procedure, including preparation of electrodes, etc., takes about 15 minutes. After developing, fixing and drying, this strip of film is placed in the projection comparator, and all detectable elements are tabulated together with the lengths between line endings of selected lines. The time for this procedure is another 15 minutes, so that in general, a complete analysis presenting all the figures necessary for a statistical treatment of the data, can be completed in half an hour.

The elements usually appearing in such an analysis of well water are Lithium, Sodium, Potassium in the alkali group: Magnesium, Calcium, Strontium, Barium in the alkaline earth group; and Copper, Boron, Aluminum, Silicon, Titanium, Vanadium, Manganese, Iron in the other groups. These are the variables to be used either directly or indirectly in correlation. The reciprocal of the length between line endings, being proportional to the intensity, which in turn is related to the concentration of the element producing the line, is the value of a particular variable to be used in correlation.

The statistical treatment of the data can be best illustrated by an example recently worked out by our laboratories. From six to eight samples from each of four known water zones were obtained and analyzed in the above indicated fashion. These represent the known data from which mean values of each of the variables can be calculated for each zone. Using these mean values, deviations from the mean can be calculated for each variable in each sample. The mean values and the deviations from the mean, indicate to what extent a variable is diagnostic. Obviously if the mean value for the potassium to sodium ratio is very different in the four zones, and the deviations from the mean in each zone are small, then this ratio is very diagnostic. If, on the other hand the mean values for a particular variable are very similar and the deviations are large, that variable will be of no value for correlation. Thus the best practice is to pick out six or eight of the most diagnostic variables and discard the rest. Here, the advantage over the chemical method is apparent, as enough variables are at the disposal of the operator to enable him to eliminate those that cannot assist in the correlation and still leave a reasonable number for actual work. Upon tabulating the above data all the preliminary work has been completed and the correlation of unknown samples can be made.

An unknown sample is analyzed in the usual manner though data need only be tabulated on the diagnostic variables. By ordinary prob-

ability theory the probability of the sample coming from each of the four zones is calculated. A typical result is that the probability of a particular water being from zone one is .69; from zone two .93; from zone three .45; from zone four .57. Hence the sample is most probably from zone two. In the above procedure a correlation has been made entirely objectively without allowing any personal inclinations to influence the result.

To check the method, samples from known zones have been submitted as unknowns to an operator. Not only was the correct correlation made unfailingly for this particular field, but the probabilities have been so much less for the wrong zones that in some well waters, even mixing should be easily detectable.

Thus the conclusion is that the spectrographic method of correlating oil well waters is far superior to chemical methods both in speed and accuracy. That the same method can be applied to cores is apparent, and that success may often result in core correlation may be inferred from the fact that this type of analysis should be more diagnostic than heavy mineral analysis.

COMPLEX REFLECTION PATTERNS AND THEIR GEOLOGIC SOURCES*

FRANK RIEBER†

ABSTRACT

A technic is shown for producing synthetic records corresponding to assumed ideal structural conditions, the resulting records being reproduced in the usual visual form, and also in the form of directionally analyzed records. Examples of folding, faulting and irregular sedimentation are treated.

The research on which this paper is based was originally undertaken by the writer several years ago. The objective aimed at was the development of a radically new attack on the problem of reflection shooting by which it was hoped that many of the limitations of the usual methods might be overcome.

Exploration by reflection shooting had progressed, at that time, to a point where these limitations had begun to be definitely evident. While it was known that the method as a whole was eminently satisfactory for outlining broad structural features in regions where clear records of reflections could be obtained, users were beginning to recognize that certain areas and certain types of structure presented problems which might be somewhat beyond the capacity of the technic as then employed—and even beyond the reach of such improvements in the apparatus and methods as might reasonably be expected.

It was known that blind spots would occur in otherwise clearly mapped areas and that within these spots only very poor records were obtained. In fact, very extensive regions could be found in which poor records were the rule, and only a few spots existed where passable records could be obtained.

And in some places, reflections could be identified only on certain parts of the records, corresponding to a certain depth zone, above and below which no satisfactory results seemed to be obtainable.

The limitations of the method could, therefore, be expressed in terms of the frequency of occurrence of unsatisfactory records, which, by the way, were usually marked "N. R."—presumably meaning "no reflections."

These poor records were roughly divisible into two types, the first

* Paper read at the Fall Meeting, Houston, Texas, November, 1936.

† Rieber Laboratory, Los Angeles, Calif.

of which certainly contained no reflections. In fact, they could not properly be said to contain anything at all. Beginning with waves of moderate amplitude, the recorded vibrations on such records would die down very rapidly to so small value that, even had reflections been present at the later portions of the record, they could certainly not be seen or identified.

To meet such conditions, it was customary to change the shooting or recording procedure or the location of the set-up, until a record of readable amplitude was obtained.

Poor records of the second class were, however, far more numerous, and presented a much more interesting problem. They contained vibrations of adequate amplitude throughout the useful length of the record, but were characterized by a great dearth or a complete absence of the characteristic pattern by which reflected waves are usually identified.

Such patterns consist of a sharply recognizable group of waves, appearing at approximately equal magnitudes and with similar wave forms on all of the adjacent traces and exhibiting a "line-up" or relative time correspondence. This characteristic appearance is so well-known to every one who has had to do with reflection shooting that it should require no further description here.

Instead of showing any of these patterns, the confused records under discussion would show what appeared to be random wave motion on all traces—frequently throughout the length of the record without even one line-up or correspondence of waves.

If any explanation of such records was attempted, it was usually to the effect that an explosion generated in the earth two classes of phenomena, definable as "reflected waves" and "random vibrations." Both of these were present everywhere to some extent, but in some places, for reasons not clearly understood, the random vibrations became very large and the reflected waves either shrunk to a very small value or were entirely absent.

When regions were encountered where such unpatterned or illegible records were obtained, and where minor modifications in shooting procedure failed to produce clearer ones, the setback was accepted in a philosophical spirit, as merely one of the minor tribulations of a not altogether tranquil profession.

Several recourses were, of course, open to a geophysicist undertaking to work a region where a majority of the records were confused. The first and most logical was to take lots of records, to mark on them

anything that might be a reflection, and then to cross-compare all of the results. By this procedure anything that was real and present in the earth should logically give evidence of its presence on a number of different records, where waves of a truly random nature would not necessarily be obtained twice in the same place. This procedure worked well enough in some places, where it was not quite impossible to mark on each record a reasonable minimum number of line-ups faintly resembling reflection patterns.

Another was to refuse to work the area entirely, giving as a reason that "no reflections could be obtained." If this were actually a fact, it could, of course, be a clinching argument against reflection work in certain places.

The above was roughly the state of reflection shooting at the time the writer undertook to develop a new method of attack. Incidentally, while it has been altered slightly by various subsequent developments, it is very largely the condition confronting the most widely used methods of reflection shooting at the present time.

At the outset of the new attack, the problem of the confused and unpatterned record was seen to be at the heart of the whole matter. Such records obviously contained in one form or another practically all of the information sent back by the earth after a shot was fired. If it could be proved that this content was nothing but random waves unrelated to geology, or so broken up that the fragmentary answers could not be translated into terms of structure, there was certainly no point in going further.

On the other hand, the fact that records of this type almost invariably occurred in connection with geological anomalies rather challenged the attention. As a specific example, such records were known to be associated with faulted regions so uniformly that they were customarily used as indirect evidence of the existence of faults.

Geophysicists cannot claim the honor of discovering that it is expedient to draw a fault through an unexplainable inconsistency in the data. Geologists have used this device too long in their own work to object seriously if it is borrowed in its entirety by a young art.

Further, a species of mental parallel makes the connection between faults and confused records seem very plausible. The waves on a confused record are jumbled. The strata in a fault zone are likewise thoroughly scrambled. It is natural to accept the connection between these two facts without inquiring too closely into exactly how the disturbance of the strata might result in disturbance of the recorded waves.

On examining into the confused record, however, it did not seem so certain that reflected waves were absent. True, if there were any reflected waves there, they did not show as patterns in the customary manner. But the presence of vibrations of good amplitude throughout the length of such records had to be accounted for in some way. It scarcely seemed possible that some mysterious random cause, unrelated to geology, and possibly concerned solely with surface conditions, could so invariably produce a confused record in the vicinity of a geological anomaly.

A much more plausible explanation was soon found—namely, that anomalies of many types return waves from the earth from several directions at the same time. While each of these waves—if the others could have been entirely suppressed—would have given a clear pattern on the record, the simultaneous arrival of all of them produced such an overlapping that the resultant combination appeared to be entirely random.

Following out this assumption, which has since been abundantly sustained by experimental and field results, the writer undertook to develop a method by which such a complicated group of returned waves could be recorded and thereafter analyzed into their component elements. In this system, the traditional seismograph form of record was abandoned entirely, and a fresh start made by recording the earth vibrations as sound tracks on a film, in a manner similar to the recording of sound for talking pictures. This film was later passed through an optical analyzer with a photo-electric cell, developed especially for this purpose, and adapted to break down groups of wave trains of mixed direction of arrival into its component wave elements, to each of which a direction of arrival and a time of arrival could be assigned.

This system has been termed the Sonograph, and has been briefly described by the writer on several previous occasions.[1] Unfortunately, however, these descriptions seemed to have been so short, and so lacking in detail, that the purpose and functioning of the new method are not widely and completely understood. For example, an impression seems to exist that if a Sonograph, and a reflection seismograph of the usual pattern, could be set up in the same locality, and used to record the same shot, they should obtain more or less interchangeable results. That is to say, if a reflected wave were recorded on the seismograph

[1] "A New Reflection System with Controlled Directional Sensitivity" by Frank Rieber, published in GEOPHYSICS, Vol. 1, No. 1, January, 1936.

with a clear and unmistakable pattern, it is readily conceded that the Sonograph record, when analyzed, should show this same reflected wave coming from the same depth and the same direction as computed from the seismograph findings.

If, on the other hand, geological conditions were such as to give a confused record on the seismograph, in which no pattern appeared, this would mean that there were really no reflected waves present in the earth. Hence, the Sonograph record when analyzed, should show no reflections.

By a simple extension of this same line of reasoning, it has been argued that if, under such conditions, a seismograph record should show no patterns of reflected waves, and if an analyzed Sonograph record showed reflections to be present, then these reflections could not be real, but must have been somehow manufactured by the analyzing instrument in the process of reproducing the record.

The following demonstration will show that this is not the case, and that an analyzed record from the Sonograph possesses, in fact, a number of advantages over the ordinary visual record.

This demonstration is arranged as an idealized comparison of the visual recording seismograph of the Sonograph methods and consists of the following steps:

First, it will be shown that most common structural anomalies must inevitably return waves from a number of different directions, and overlapping each other with respect to time. These waves may be thought of either in terms of "central rays," commonly used in diagrams of optical reflection, or they may be considered in terms of the shape of their emerging wave fronts at the time when these wave fronts reach the surface of the earth near the receptors. For the purposes of brevity, waves of this class arriving from various directions over the same time range, will be termed "criss cross" waves.

The second step in the demonstration is to show a method by which such criss cross waves can be artificially produced in the laboratory, and thereafter recorded

(a) As seismograph records of the usual form,

(b) As analyzed Sonograph records.

The third step in the demonstration will be to take an assumed and extremely simple geological condition from which criss cross waves will be produced, and to make, by the above technic, equivalent seismograph and Sonograph records of this criss cross pattern.

A comparison of the two methods is then possible, in that an at-

tempt may be made to reconstruct the original geological condition from the seismograph record by the usual visual identification of waves and a similar attempt may be made using the analyzed Sonograph record.

The following diagrams are intended to illustrate a few of the many geologic conditions which can cause criss cross waves. The diagrams have been purposely simplified to their bare essentials, and also the structural irregularities have been exaggerated to give clearer emphasis to the criss crossing of the wave paths.

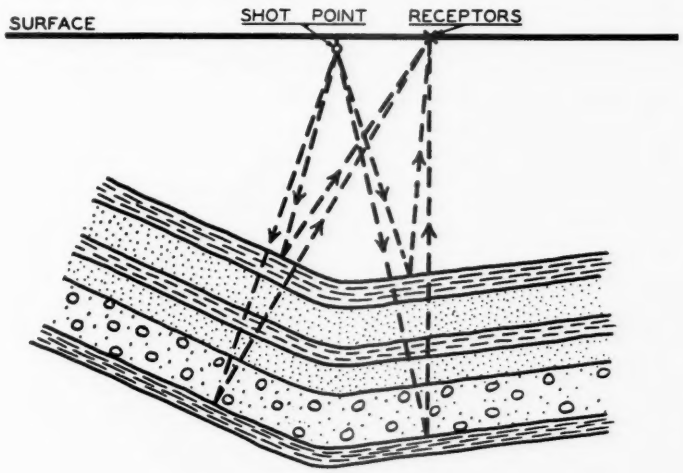

FIG. 1

A simple, sharply folded syncline has been chosen for this demonstration and is shown in Fig. 1. The shot point and the receptors are presumed to be set up over the axis of the fold as shown. Under these conditions, it is quite apparent that waves will be returned by the structure from two directions at once as is indicated clearly by the wave paths in the diagram.

Fig. 2 shows another commonly encountered structural condition causing criss crossed waves which are again indicated by the wave paths on the sketch. Crenulated beds of this type, and other similar irregularities in sedimentation, are recognized as occurring very frequently where deposition has taken place under shore line conditions.

Fig. 3 shows a combination of overlap and truncation, the wave path diagrams clearly illustrating the criss crossing of the waves. In passing, it might be mentioned that all of these conditions have been

purposely exaggerated as to dip, in order to clearly separate and indicate the individual wave paths.

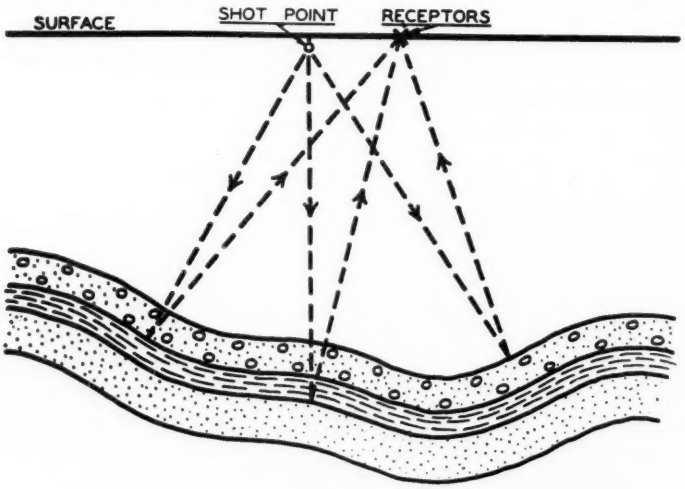

FIG. 2

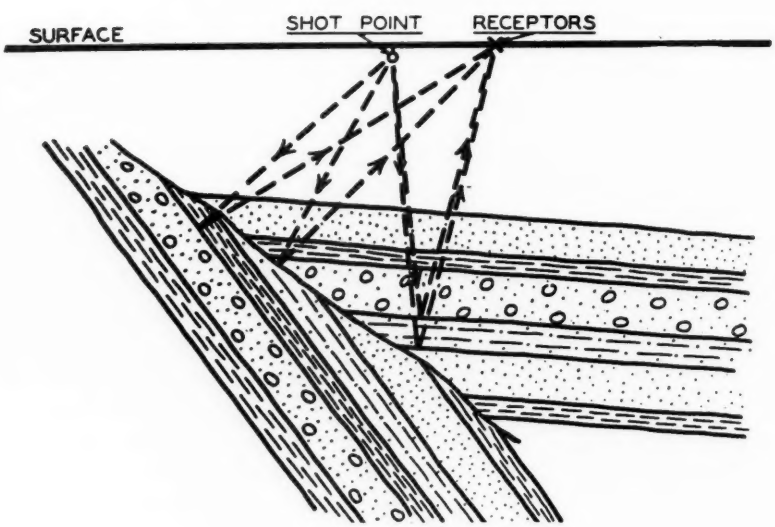

FIG. 3

Fig. 4 shows a normal fault in a homocline, and illustrates one of the reasons why fault mapping by the ordinary reflection technic must frequently be done in a negative fashion. The heavy wave paths

refer to reflected waves of the usual sort coming from successive strata, while the lighter wave paths refer to waves returned from the breaks and discontinuities in the fault region itself.

As will shortly be shown, these waves returning from the fault are not always to be considered as reflections. Diffracted waves are frequently sent back from fault zones, although their presence has not

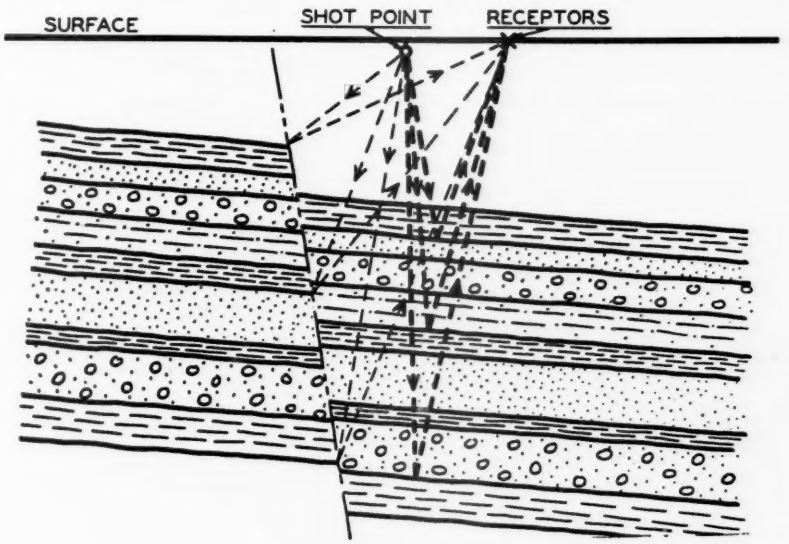

FIG. 4

heretofore been generally recognized due to the impossibility of separately identifying them on the usual form of records.

To illustrate this point more clearly, Fig. 5 shows a similar faulted condition, in which the dotted line method of indicating wave paths has been abandoned in favor of diagramming the actual wave fronts of the downgoing and returning waves.

The writer recently suggested an interesting method by which the shape of returning wave fronts could be visually demonstrated[2] and the existence of diffracted waves from faults could be clearly shown. The apparatus and technic for this purpose were adapted from previous similar work by various acoustical engineers, who employed it for tracing wave patterns in auditoriums and the like. It consists essentially in creating a miniature explosion in air, from which a sharp

[2] "Visual Presentation of Elastic Wave Patterns Under Various Structural Conditions," by Frank Rieber, GEOPHYSICS, Vol. 1, No. 2, July, 1936.

wave front is radiated and thereafter photographing this wave front at various stages of its progress. If a model of any structural condition

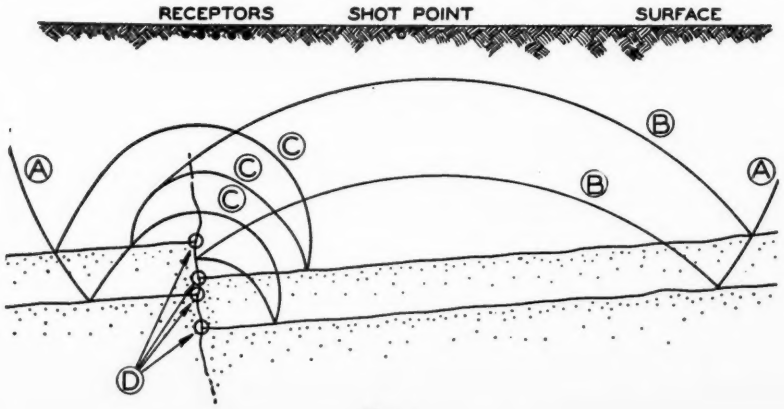

FIG. 5

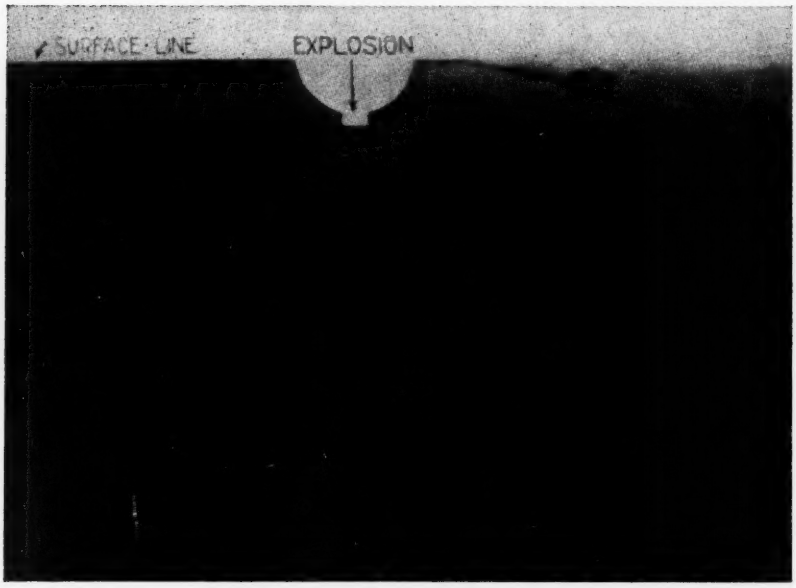

FIG. 6

is interposed in the wave path, the reflected and diffracted waves will be clearly demonstrated. The form of this apparatus constructed under the writer's direction has been termed a strobograph.[3]

[3] "Sound Wave Stroboscope," by B. F. McNamee, "Electronics," November, 1936.

Fig. 6 shows an actual group of wave fronts photographed in connection with a model of a single faulted stratum, and will be seen to correspond strikingly with the theoretical diagram in Fig. 5. A criss cross wave pattern from the faulted model is about to emerge at the surface of the illustration. Obviously, if a succession of such strata exist in the earth, and the same fault condition exists in all of them, a succession of these criss cross waves must return.

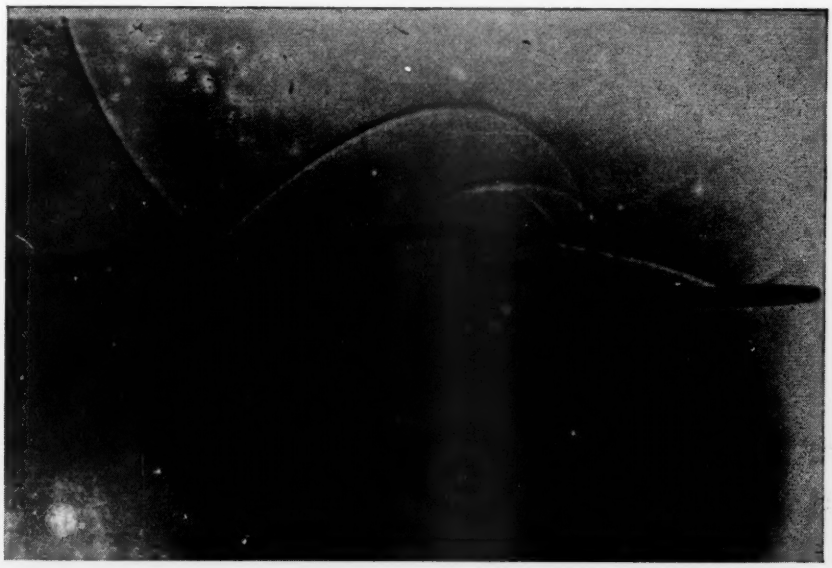

FIG. 7

Fig. 7 shows a model of a double step fault in a stratum as recorded by the strobograph. The criss crossing of reflected and diffracted waves on this picture scarcely needs further comment. If several such strata followed one another at successive depths, the complexity of the emerging patterns can readily be visualized.

The structural conditions shown in the preceding diagrams are, of course, thought of as existing only in the plane of the drawing. Therefore, they indicate only a small fraction of the possibilities for criss crossing of waves under actual field conditions. A three dimensional earth will, of course, return waves to the surface from all possible directions, and far greater confusion will exist than can be shown in any two-dimensional sketch.

A technic will now be shown by which criss cross waves of any desired and predetermined pattern may be produced in the laboratory, and reproduced either in the usual visual form common to seismograph records; or in the analyzed form used in the Sonograph.

As the first step in this process, an electrical source of wave trains was constructed. Transient wave generators of this type are well known to geophysical engineers, and are commonly used to make various tests on instruments.

Using this wave train source, a succession of sound track records are made, as illustrated in Fig. 8, which shows ten such sound tracks on a single film. Similar waves are recorded on all ten tracks,

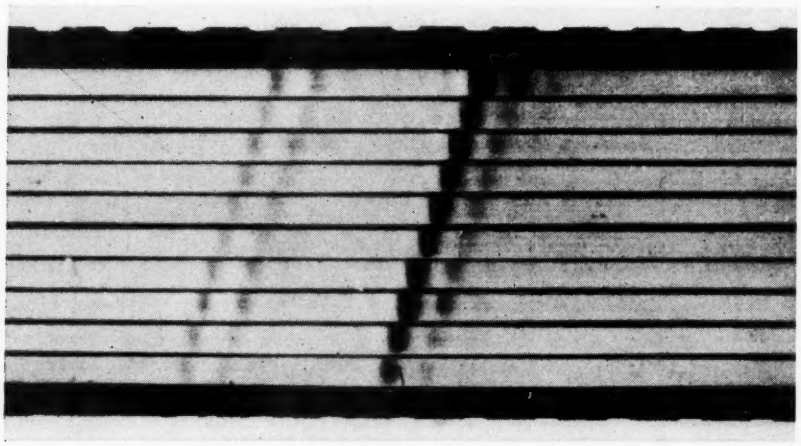

FIG. 8

but at successively later times, giving a "line-up" corresponding to the arrival of a wave in the earth, as recorded from a succession of receptors.

Such a film record has the advantage that it may thereafter be reproduced either as a visual type seismograph record, or through the Sonograph analyzer, thus permitting a direct comparison of the relative ability of the visual and the analyzed method in recognizing and identifiying individual wave trains.

Reproduction in the usual visual form consists in converting each individual sound track into a corresponding oscillograph trace. The record shown in Fig. 8 has been thus reproduced in Fig. 9, which may be thought of as representing a single isolated and idealized re-

flected wave, as it would appear on a seismograph record if all other vibrations could in some way be eliminated.

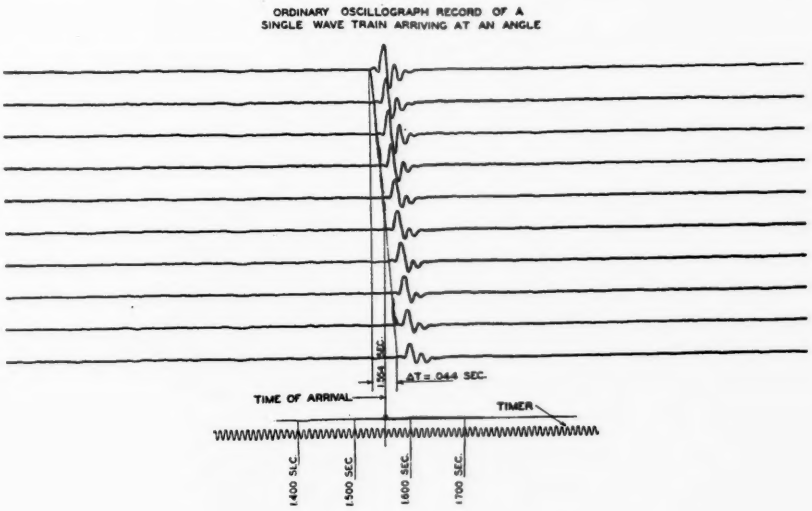

FIG. 9

Having shown the simple wave train in film form in Fig. 8, and in the usual seismograph record form in Fig. 9, we can now proceed

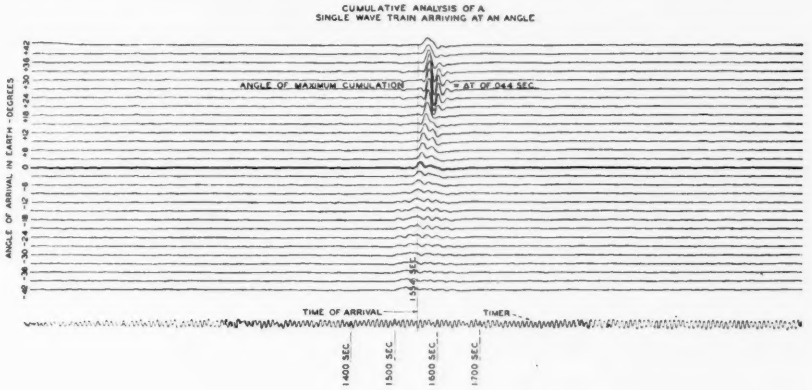
CUMULATIVE ANALYSIS OF A
SINGLE WAVE TRAIN ARRIVING AT AN ANGLE
ANGLE OF MAXIMUM CUMULATION
= ΔT OF 044 SEC.
ANGLE OF ARRIVAL IN EARTH - DEGREES
-42 -36 -30 -24 -18 -12 -6 0 +6 +12 +18 +24 +30 +36 +42
1554 SEC.
TIME OF ARRIVAL
TIMER
1400 SEC.
1500 SEC.
1600 SEC.
1700 SEC.

FIG. 10

to the appearance of the same wave train as reproduced through the Sonograph analyzer as shown in Fig. 10. This form of presenting

the wave is termed an analyzer strip, and will be seen to differ radically in appearance from previous conceptions of wave records. Each trace on this analyzer strip represents a different angular setting of the analyzer, corresponding to a known direction of arrival in the earth. These assumed angles of arrival are shown in the scale at the left of the figure. To read the record, the presence of an outstanding wave group is first noted (a simple matter in the figure shown, which contains only one wave group) and the successive traces on the ana-

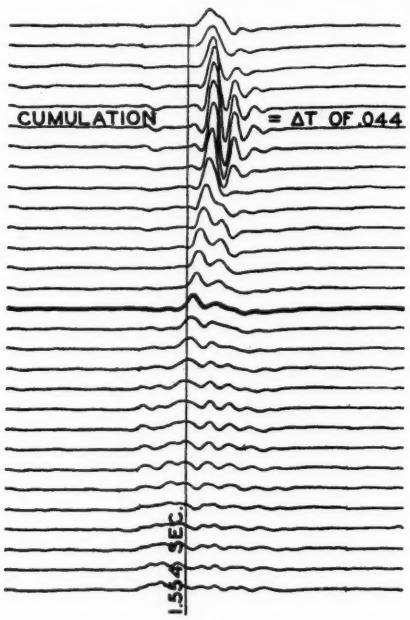

FIG. 11

lyzer strip are then examined to find that on which the wave group reaches its greatest amplitude. This trace is selected and followed back to the scale at the left of the figure, from which the angle of arrival of the wave is determined.

Fig. 11 shows a close-up of such a record, from which the eixistence of a rising and falling amplitude on the successive traces, and the possibility of selecting a location of maximum amplitude, may be clearly seen. Each of the foregoing three figures represent the same event, namely, a single arriving wave train. They may be thought of as three different languages for expressing the same fact.

Having shown how a single wave train can be recorded in a form similar to a reflected wave pattern, it will be seen that a succession of such wave trains can readily be made by the same method, corre-

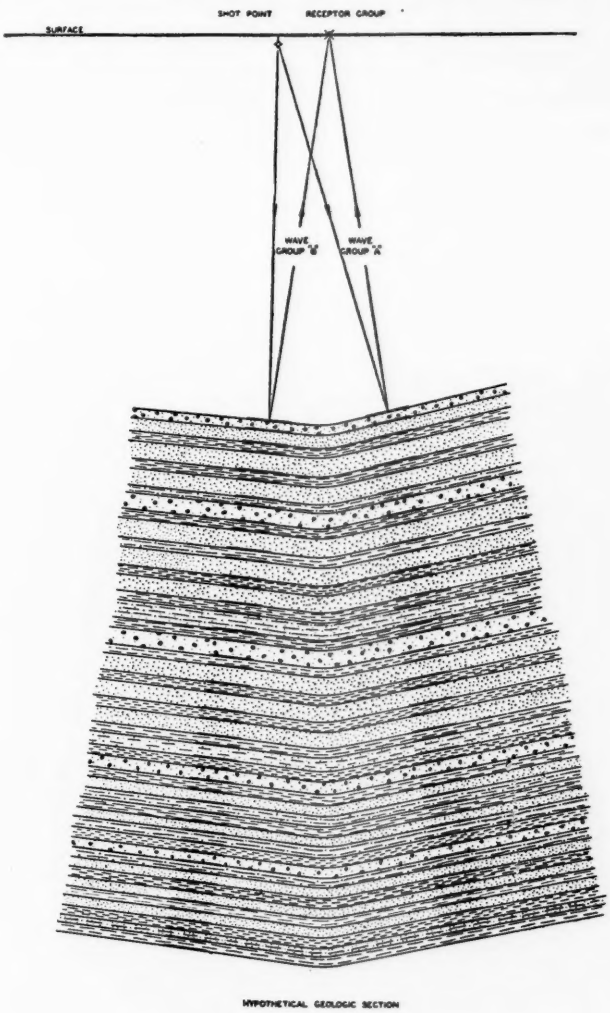

FIG. 12

sponding to successive reflections arising from a series of parallel beds. Such wave trains will hereafter be termed "synthetic reflections."

Having prepared a film containing a series of synthetic reflections

from some one bed sequence, a second film may be as readily made, containing a similar set of synthetic reflections from some other group of assumed beds. Both of these films may then be combined, by a process of re-recording similar to that used in motion picture work, where it is termed dubbing, and used to combine various sound effects originally recorded separately. The re-recording apparatus used by the writer is capable of combining three separate sets of reflected waves into one complex pattern.

Having sufficiently described the technic employed, the following demonstration can now be given. We will begin with the assumed structure shown in Fig. 12, a simple chevron syncline, which has

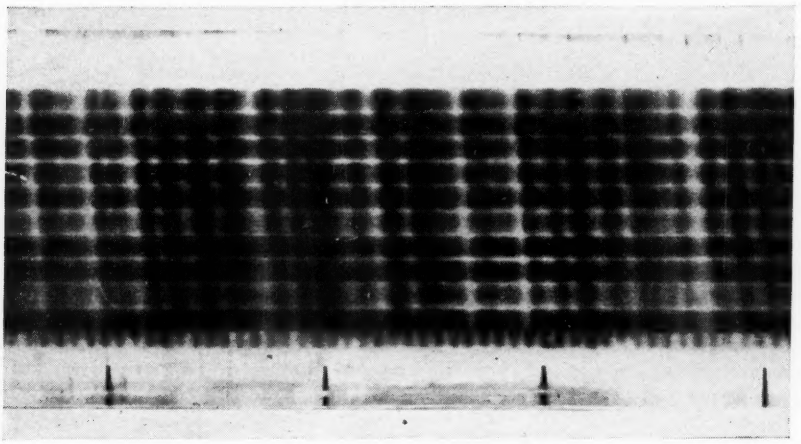

FIG. 13

been idealized for the purpose of simplifying and emphasizing the demonstration. It is assumed that each interface between the strata will give a reflected wave, and the location on each stratum from which the reflections will be returned to the receptor is indicated by a slightly heavier line.

Wave paths for the reflections from the first stratum only are indicated. Further, the succession of reflections from the two limbs of the syncline are divided into one group A and one group B, respectively, for purposes of reference.

Separate film records for wave group A and wave group B are next made in accordance with the technic just described.

Fig. 13 shows a close-up of the film for group A, on which the

succession of wave trains corresponding to the various strata may be clearly seen by eye.

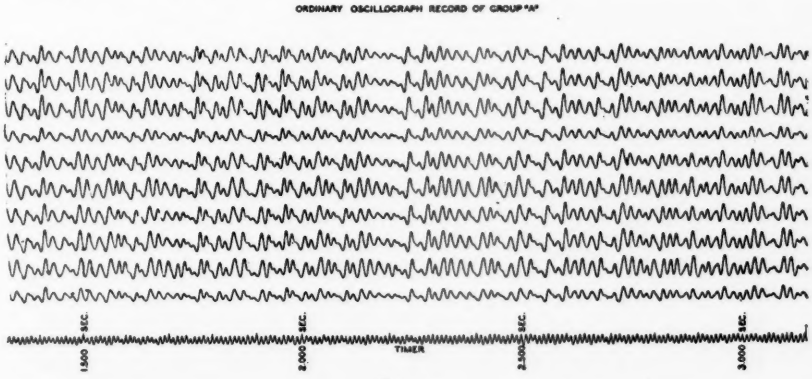

FIG. 14

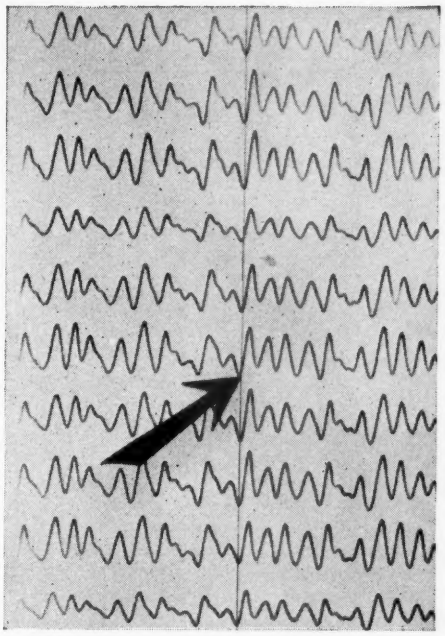

FIG. 15

Fig. 14 shows this film translated into the usual seismograph form, which corresponds roughly to the type of record which would

be obtained over the syncline if, by some miracle, one side of the structure could have been removed or eliminated and reflections from the

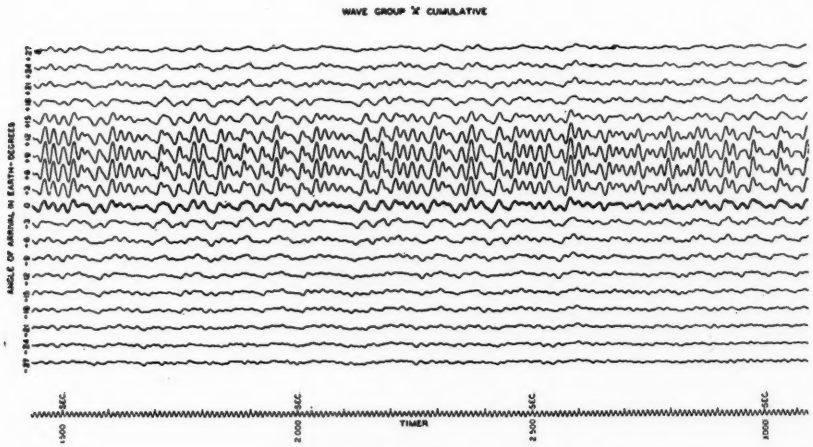

FIG. 16

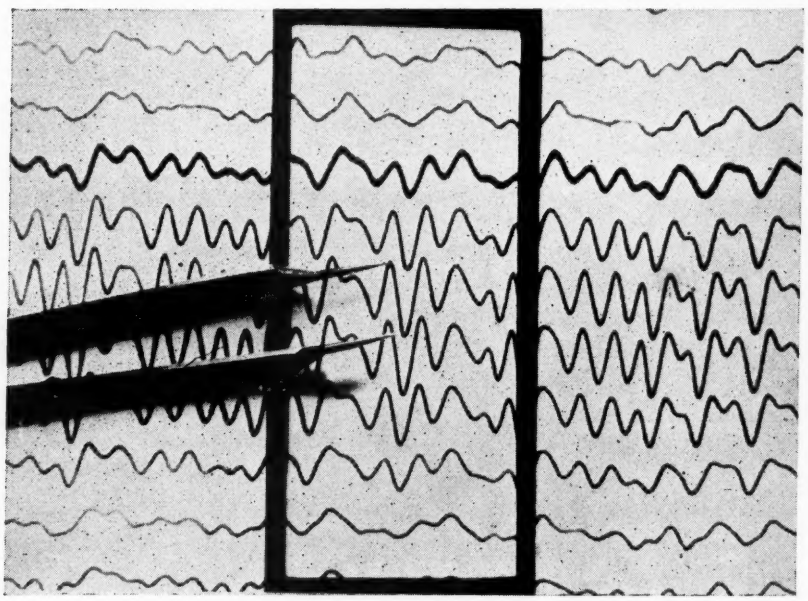

FIG. 17

other side only could be considered. While the wave forms produced by the transient source are not identical with those appearing on

many seismograph records, especially those in which special filtering and turning circuits are employed, it will be seen from inspecting Fig. 14 that there should be no difficulty in finding any number of line-ups and deducing the presence of the corresponding reflected waves.

Fig. 15 shows a close-up of this same record, in which the sharpness of the line-up may be more clearly evident. Given such a record there would, in practice, be no difficulty in reconstructing from it a close approximation to one limb of the syncline.

Fig. 16 shows the Sonograph analyzer strip corresponding to the seismograph record in Fig. 14.

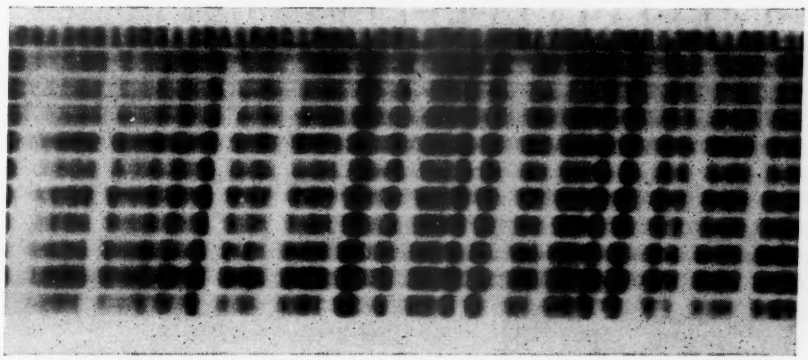

FIG. 18

Fig. 17 is a close-up of the same wave train shown in Fig. 16, to illustrate the positive manner in which the maximum trace may be selected.

The dividers shown in the picture mark one of the maximum waves. It will be seen that by the use of the Sonograph it would likewise be possible to reconstruct one limb only of the syncline, and that so far as the two method are entirely comparable.

We will now assume that the geological miracle just performed can be reversed, and that we can eliminate that side of the syncline containing wave group A, and deal solely with the other side. Fig. 18 shows the film for wave B group alone.

Fig. 19 shows this film reproduced in a manner corresponding to the ordinary seismograph record.

Fig 20 is a close-up of one of the line-ups. Obviously, the seismograph type of record would permit the reconstruction of that part

of the structure defined by wave group B, if wave group A were only absent.

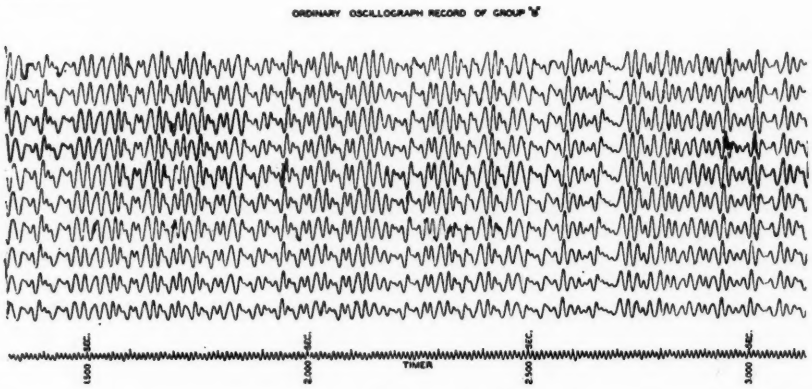

FIG. 19

Fig. 21 shows the analyzer strip for wave group B and Fig. 22 shows a close-up of a portion of this strip—clearly indicating that

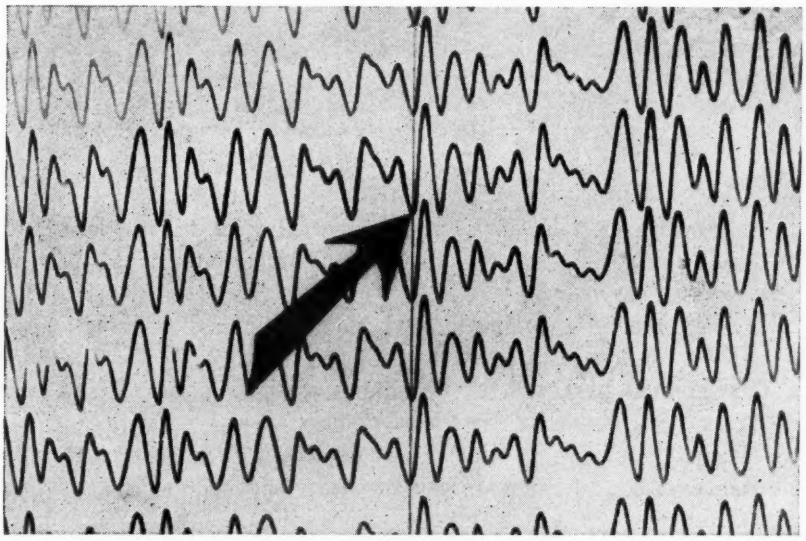

FIG. 20

the maximum waves can again be picked, and their arrival time and directions readily determined from the analyzer strip. Again little

choice is left between the seismograph and Sonograph methods. Either could have reproduced the second limb of the syncline.

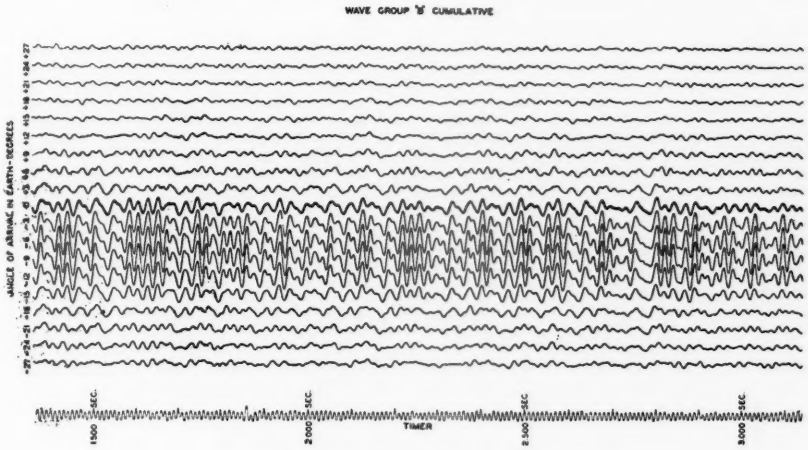

FIG. 21

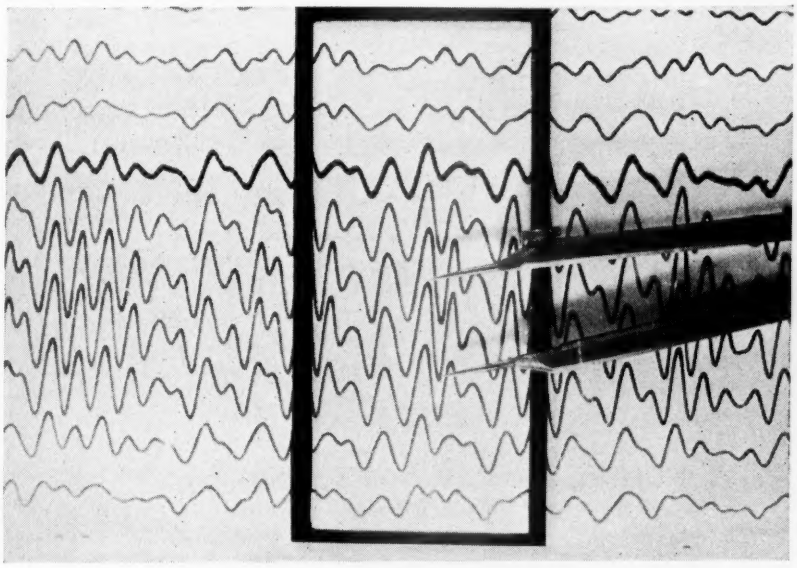

FIG. 22

Fig. 23 is a record showing the actual condition which would exist in the earth with both limbs of the syncline active in returning

reflections. The miracle has been repealed, and the true criss cross wave pattern is now being recorded.

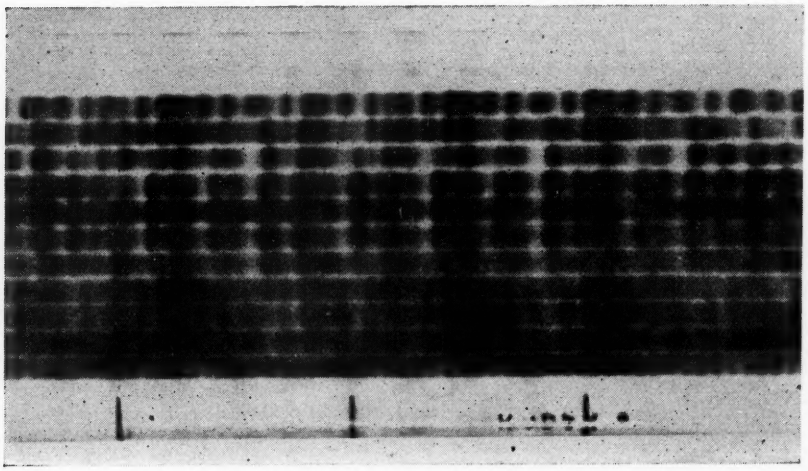

FIG. 23

Fig. 24 shows this same record reproduced in seismograph form. It will at once be seen that practically no line-ups of a quality usually acceptable for plotting are present in this record.

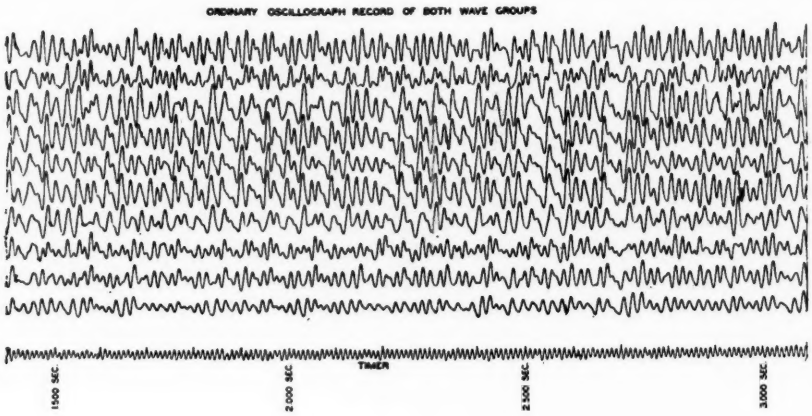

FIG. 24

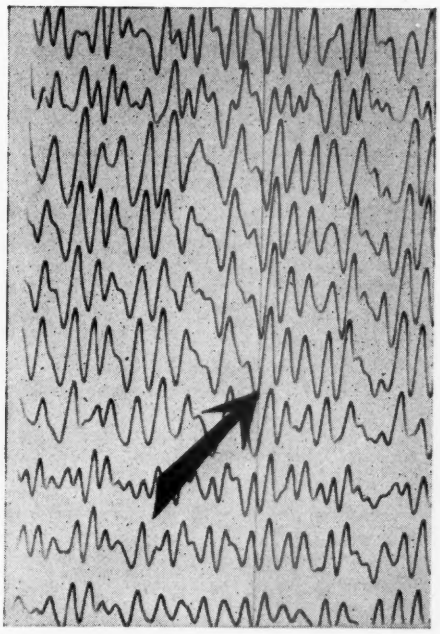

FIG. 25

In Fig. 25 the arrow points to one approximate line-up which is shown in a close-up, and will be seen to be of a very dubious nature, considered from the point of view of the usual critical computer.

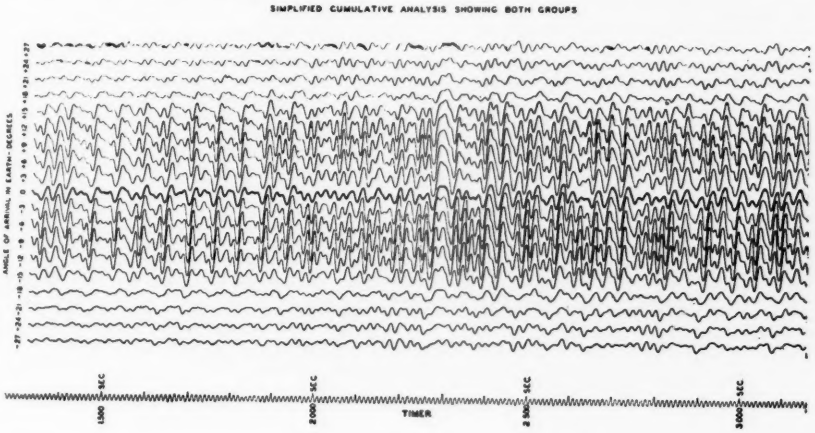

FIG. 26

Fig. 26, on the other hand, shows this same record reproduced through the Sonograph analyzer. Wave group A and wave group B will be clearly seen as separate entities.

FIG. 27

The actual record given in Fig. 26 was submitted to an experienced computer on Sonograph records, who reconstructed from it the cross section shown in Fig. 27. It will be noted that the reconstructed sec-

tion does not correspond completely with the original assumed section. Neither the dips of the successive strata nor their positions, are identical with those of the original. This deviation will give some idea of the limits of error of the Sonograph when applied to such a thoroughly interlaced group of criss cross waves.

This test—which, incidentally, has been made for a wide variety of other structural conditions, should demonstrate clearly that the Sonograph analyzer is more than a mere mechanical equivalent for the earlier visual identification of waves on seismograph records. The

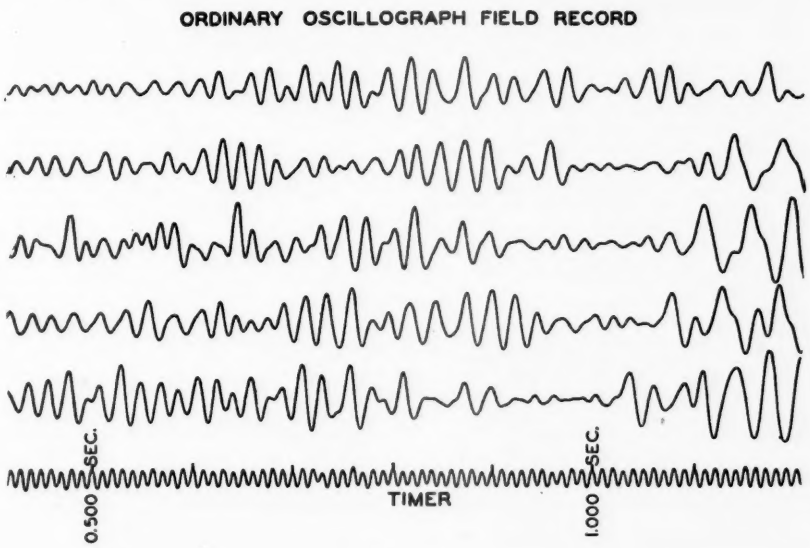

FIG. 28

section just reconstructed could certainly not have been filled in with equal completeness and quality by picking out line-ups and waves on the record in Fig. 24, in the usual manner.

In closing, it may be remarked that the use of the Sonograph in the field has now extended over a sufficient period of time, and covered a sufficient variety of problems, to completely confirm its utility in separating and identifying criss cross waves. Sections have been successfully plotted by the method in any number of locations where visual records showed no usable reflection patterns. And these sections made by the Sonograph have been checked in a very satisfactory way, and in a great majority of instances, against other geological and subsurface information.

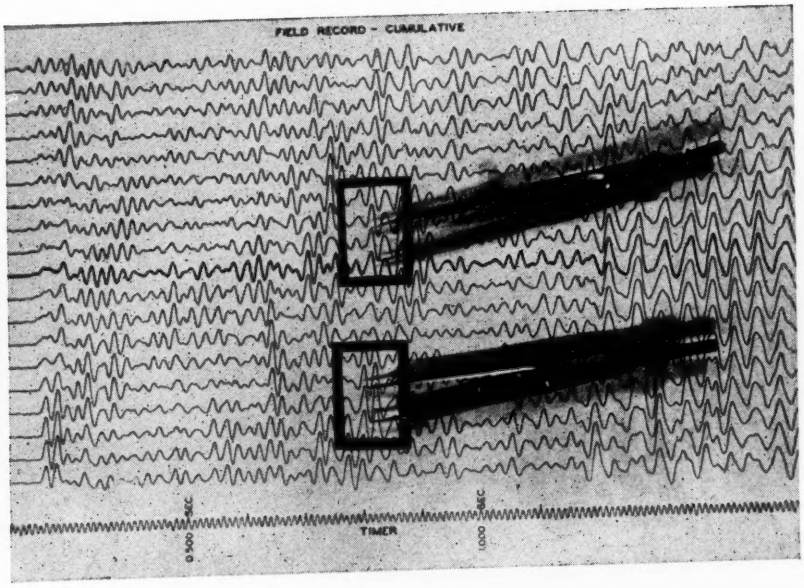

FIG. 29

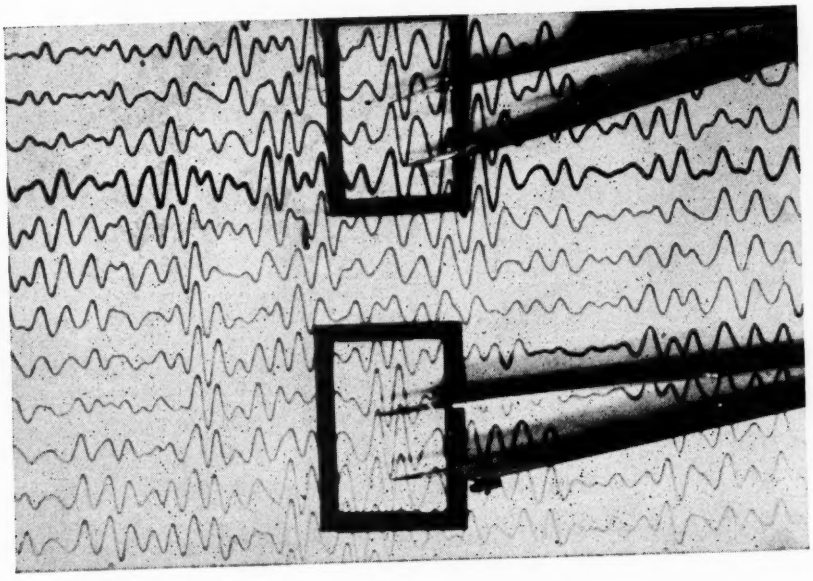

FIG. 30

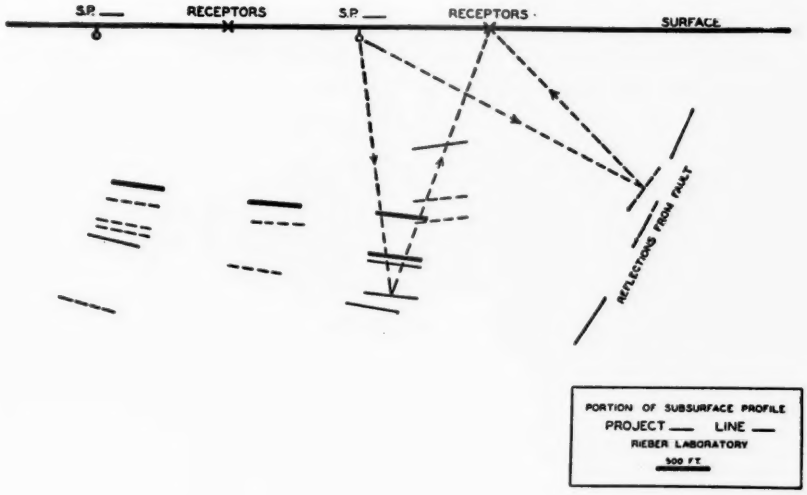

FIG. 31

As a matter of interest, Fig. 28 shows a typical Sonograph film, taken on an actual structure, but reproduced in the customary seismograph form. Little evidence of pattern or line-up will be seen.

Fig. 29 shows the same film reproduced through the Sonograph

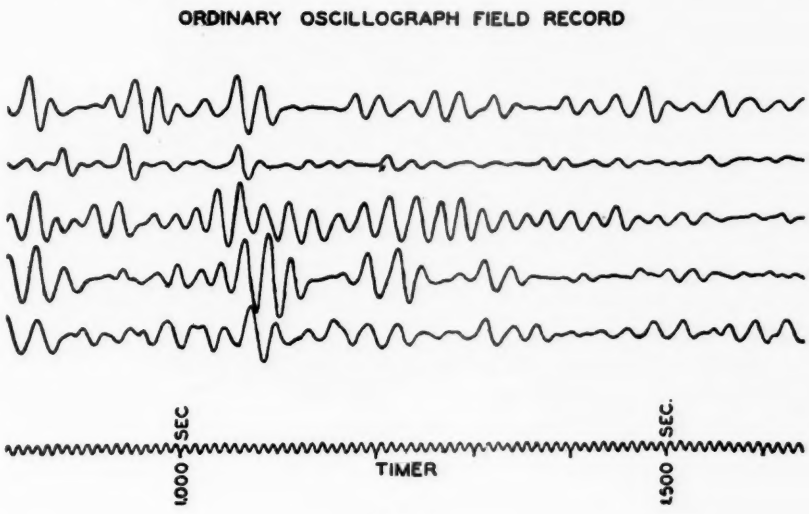

FIG. 32

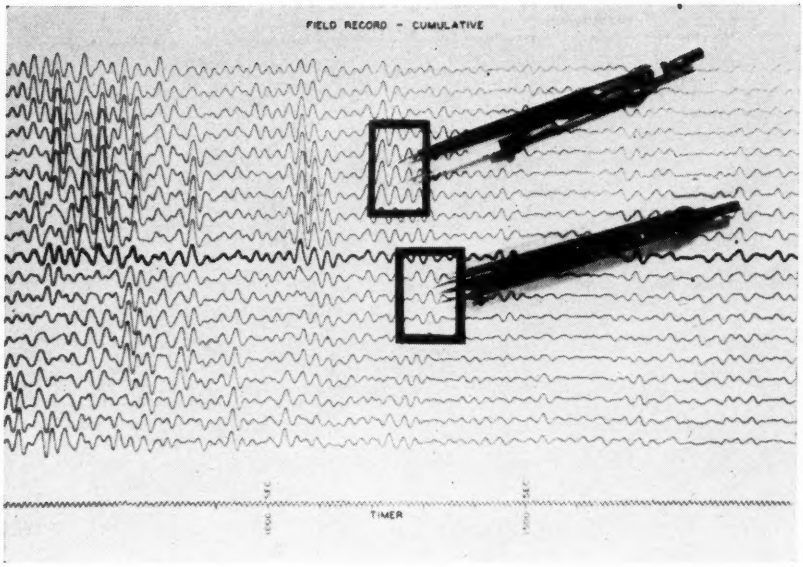

FIG. 33

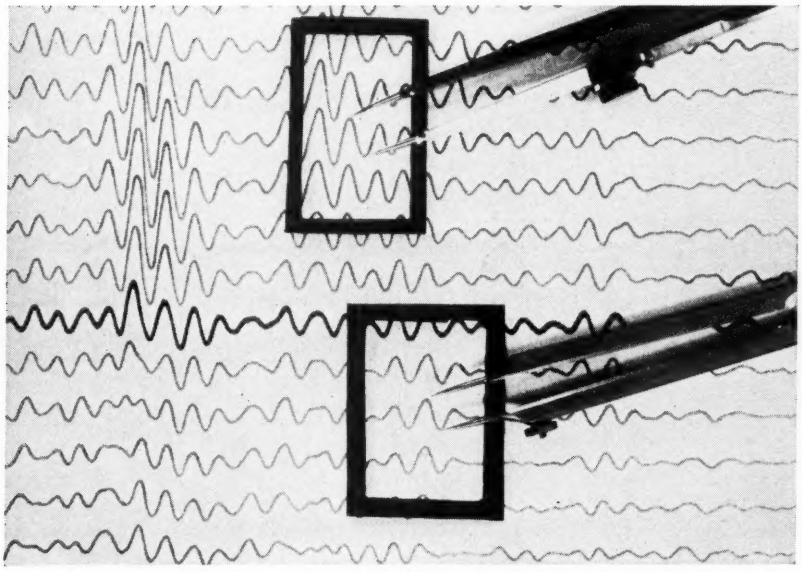

FIG. 34

analyzer. Dividers mark two of the many individual wave trains visible on this record, on their points of maximum amplitude.

Fig. 30 shows the close-up of this divider and the two wave trains to illustrate the nature of the maximum picked.

FIG. 35

Fig. 31 shows a reconstructed section in the earth, made from a number of successive shot points, on which the positions of the two individually marked waves from Fig. 30 are indicated by the wave travel paths. Incidentally, the fault shown in this section was amply confirmed by local surface evidence.

Fig. 32 shows another Sonograph record, reproduced from the film in the ordinary seismograph style. Again no striking line-ups will be seen.

Fig. 33 shows the result of passing the same film through the Sonograph analyzer. Again, two of the numerous wave trains are indicated by two pairs of dividers shown in a close-up in Fig. 34.

Fig. 35 shows a plotted cross-section of which the preceding record was a part. A presence of a fault in this region is clearly demonstrated. Arrows A and C show probable reflections from some elements of the faulted region, while arrows B, D and E indicate probable diffractions.

In conclusion, while it is possible that all of the causes of confused records have not been identified at the present time, it seems likely that most of them result from overlapping of wave trains arriving from several directions at the same time. Therefore, although such records are customarily regarded as containing no reflections, the actual trouble is that they contain too many reflections.

Naturally, confusion of this sort reaches its maximum when the various interfering waves are of approximately the same magnitude, and each obliterates the pattern of the other.

There is, however, another serious result of criss crossing which is not so immediately apparent. It is found frequently near faults, where the reflected waves from strata arrive from one direction, and diffracted waves from a fault arrive from a different direction. Often the reflections from the strata are of large magnitude, while diffracted waves are weak. Therefore, the weaker waves do not interfere appreciably with the recording of the stronger ones, and a record is obtained which seems to show merely normal stratification. The weak waves from the fault are missed entirely on visual inspection, and hence are not mapped.

Sonograph records, under conditions just described, frequently permit the separation of even very weak waves arriving from unusual directions, and hence make possible the identification of faults whose presence is not evident on the usual reflection records.

ATMOSPHERIC POTENTIAL GRADIENT MEASUREMENTS*

LYNN G. HOWELL†

ABSTRACT

Relative atmospheric potential gradient measurements were made in the neighborhood of a salt dome. Two sets of measurements were made simultaneously at two different points. Wulf-type electrometers were used with thorium ionium nitrate collectors insulated with sulphur. A number of set-ups were made and no significant variations in potential gradient were found.

INTRODUCTION

The surface of the earth is the seat of a negative charge[1] which is being supplied at a rapid rate from an unknown source. This charge is conducted off by an ionic current in the atmosphere, the current density of which is of the magnitude of 2×10^{-16} amperes per square centimeter. This current density integrated over the surface of the earth gives a total current of about 1,000 amperes. The potential gradient set up in the atmosphere amounts to about 100 volts per meter at the earth's surface and decreases with altitude until, for example, at 6 kilometers it amounts to only 8 volts per meter.

The potential gradient is continually changing and may even reverse in direction. At any given place the gradient goes through a fairly definite diurnal cycle[2] when averages are made over a period of time of a month or so. Since the gradient changes, absolute measurements can not be used for comparing the gradient at two points. For this reason, measurements must be made simultaneously at the two points.

APPARATUS

For the purpose of comparing the values of the gradient over an area in the field, two sets of apparatus were constructed. Each apparatus consists of a collector which is insulated from the ground and assumes the electric potential of the air at a height of about one meter from the surface of the ground, and a Wulf electrometer which measures the potential difference between the collector and the ground.

* Presented at the Annual Meeting, Los Angeles, March, 1937.

† Geophysics Research Department, Humble Oil and Refining Co., Houston, Texas.

[1] Humphreys, Physics of the Air, pp. 382–390.

[2] Parkinson, Terr. Magn., 33, 15 (1928).
Thomson, Terr. Magn., 29, 97 (1924).
Manchly, Terr. Magn., 28, 61 (1923).

The collector consists of a brass disk covered on one surface with about five or ten milligrams of thorium ionium nitrate (which is 8 to 10 per cent ionium) embedded in a clear lacquer. Ionium[3] has been used for a number of years on collectors, since it ionizes only a small volume of air due to its having only alpha ray activity of short range. The disk is mounted in a horizontal plane with the active surface facing upward on the top of a vertical $\frac{3}{8}$-inch brass tube at a height of $40\frac{3}{4}$ inches from the ground. The tube is divided by a sulphur insula-

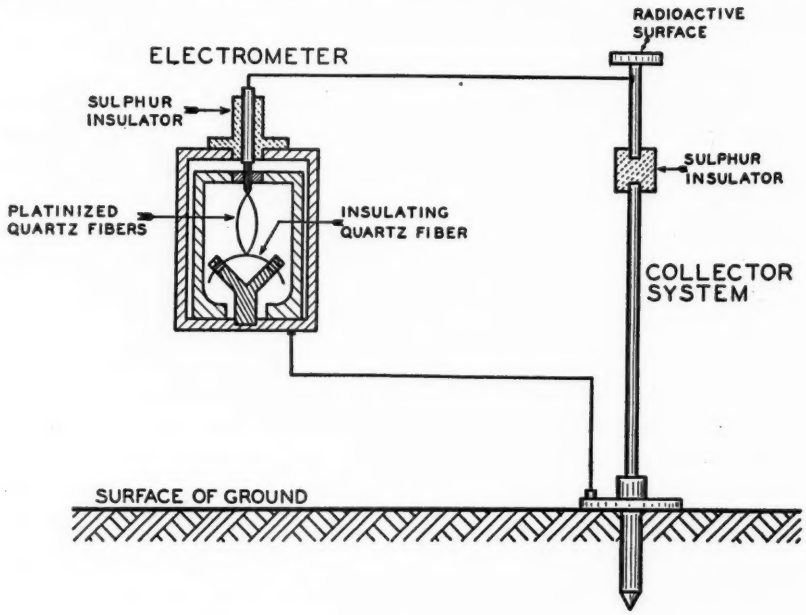

FIG. 1

tor, the center of which is $30\frac{3}{8}$ inches from the ground. The brass tube holding the collector is mounted in a steel stake driven into the ground.

The collector is connected by a fine copper wire to a binding post on the electrometer. The binding post is mounted in sulphur which insulates the post from the case of the electrometer. The post extends through the sulphur to the inside of the electrometer and is connected to the fiber suspension. The platinized fiber suspension, the deflection of which is read by a microscope, is insulated from the case at one end by a quartz-fiber bow. At the other end of the suspension, in one

[3] Bergwitz, Ph. Zs., 12, 83 (1911).

electrometer the insulation is amber, while in the other it is quartz. In one electrometer the suspension is mounted in a brass and steel frame; in the other it is mounted in a brass and invar frame, the vertical supports being made of invar to prevent differential thermal expansion with respect to the quartz-fiber suspension. The electrometers are dried on the inside with calcium chloride. Fig. 1 shows a schematic diagram of the collector connected to the electrometer.

MEASUREMENTS

In the field, measurements must be made at spots where the electric field in the atmosphere is distorted as little as possible. In other words, the equipotential surfaces over the spot should be horizontal and plane as nearly as possible. Thus, a station should not be established too close to poles, trees and other disturbing objects. In the immediate neighborhood of the collector the weeds were cleared away over an area of radius of about twenty-five feet. The ground was scraped fairly clean over an area of radius of about four feet. The collector was mounted on this bare area.

In the beginning, readings were taken at a fixed base station, while readings were taken simultaneously at various field stations. At the field station the readings of the electrometer were taken inside of a car, parked at a distance of 30 feet from the collector. At the base station the electrometer was mounted on a table at a distance of about 35 feet from the collector. These collector systems had a charging time of the order of two minutes. Calibrations of the electrometers were taken at intervals with the help of a bank of $22\frac{1}{2}$-volt batteries and a voltmeter. It was found that the calibration of the electrometer at the base station varied due to the sunshine falling directly on the instrument. Therefore, a wooden cover was made for this instrument.

In case the potential gradient became too large for the range of the electrometer, the bank of batteries could be inserted between the ground and the case of the electrometer in such a manner that the difference of potential between the case and the collector was decreased.

The measurements were made at Blue Ridge where there is a shallow salt dome. Usually, readings were taken at intervals of 30 seconds for a period of 10 minutes. The average was taken for each set of readings. It was found that in general the average gradient measured at the field station agreed with that at the base station within a few per cent when the field station was within a radius of

about a quarter of a mile from the base station. The agreement seemed to depend to a large extent on how stable the potential gradient remains during the time of observation. Thus, when local atmospheric-electric disturbances take place the gradients may differ appreciably, even, as found later, when measured at points separated by only a few hundred feet.

Next, measurements were taken with the electrometers in two automobiles, when readings could be made along a line by stepping

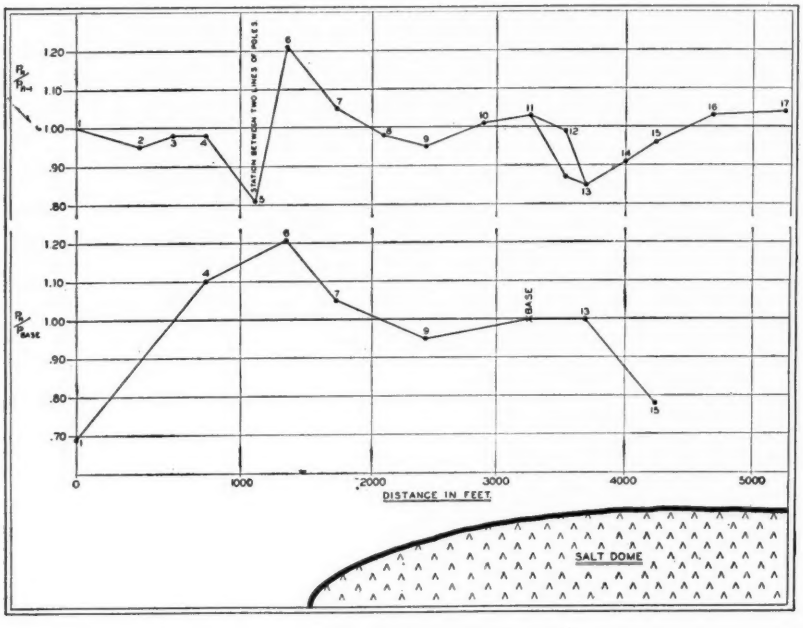

FIG. 2

across country first with one car and then with the other. Thus, except for the first and last stations, two sets of readings were taken at each station. Usually, the distance between stations was of the order of 500 feet. It was found that for the most part there was little difference in gradient between stations. Appreciable differences between stations were observed over a period of about two hours when the gradient was changing rapidly.

This line survey was run across the flank of the dome not far from producing wells. Fig. 2 shows a plot of the results. The lower curve

shows the values of the ratio of the potential gradient P_n measured at a station n (the station number being marked beside the point) to the gradient P_{base} measured at the base station. It will be noticed that the ratio deviates more from 1.00 at the longer distances. The upper curve shows the values of the ratio of P_n measured at the particular station n to the value of the gradient P_{n-1} measured at the station $n-1$. The effect of the two lines of poles on the two readings at Station 5 is quite noticeable in the values of the ratio at Stations 5 and 6. Otherwise the values of the ratio remain as close to unity as would be expected.

CONCLUSION

From the results, it is concluded that if there is a change in potential gradient of the atmosphere due to subsurface features this change is very small, and certainly small in comparison to the changes produced in the atmosphere itself. This seems quite reasonable in view of the fact that the ground is for practical purposes a perfect conductor compared to the air, with a few possible exceptions such as dry sand.[4] The change from point to point of radioactivity, humidity, dust, etc. which might influence the gradient probably depends very little on the subsurface and is probably small on the average between points not too far separated.

This work was done at the suggestion of Dr. L. W. Blau. Mr. Alex Frosch assisted in making the measurements.

[4] Johnston, Terr. Magn., 31, 145 (1926).

DISCUSSION

Discussion of a review by E. E. Rosaire (GEOPHYSICS, Vol. II, No. 1, January, 1937, pp. 63–67).

Considerable space is spent by Rosaire in charging geologists with undue delay in recognizing the importance of faulting in the structure of the Mexia and Currie (not Conroe) oilfields.

In 1923–25, geologists (among them, I and my assistants on the Rycade) working for different companies developed several parallel techniques for differentiation of the Midway, upper Navarro, and lower Wilcox into many mappable sets of beds, and did a precise job of mapping the surface geology and the surface expression of those faults. But without the knowledge, which was not developed until 1923, the presence of the faults could be recognized only at widely scattered, exceptionally favorable exposures, the throws of the faults could not be recognized, and the large, important faults would look as unimportant as the many, actually unimportant, small faults. The "faults of small throw" mentioned by Deussen very likely may have been some of those faults now known to have throws of several hundred feet.

Deussen (U. S. Geol. Surv. Water Supply Paper 335, 1914), having observed several faults "of small throw" on a line more or less parallel to, and just up-dip west of, the gas fields, made the speculative surmise that "this fault [the several faults of small throw] has broken the continuity of some of the sand lenses that occur in the Navarro formation of the Cretaceous and it is probable that the gas in the Mexia district is struck in these discontinuous and locally warped sands on the east side of the fault." Water Supply Paper 335 covers all of Texas east of the Brazos River and the outcrop of the Cretaceous, an area "as great as that of the State of Indiana," and is based on field work done by the writer during the summers of 1907 and 1908" (p. 13). No study was made of the gas fields. The surmise has been proved since then to be not far from correct, but it was based on scanty sketch data observed in hasty reconnaissance of a large area. Until the data of the many wells of the Woodbine sand became available in 1922, and until the data of the precise surface mapping became available in 1923–24, that surmise was only an interesting suggestion, which quickly seemed to be controverted by more abundant and more reliable data.

In 1915–16, careful study of the Mexia gas fields seemed definitely to indicate that they were on elongated anticlines. In 1915, Clapp for the Lone Star Gas Company and Matson for the U. S. Geological Survey (Bull. 629, 1916) both studied the well data and agreed in interpreting the gas accumulation as occupying the crest of an elongated anticline in the Nacatoch sand. Even in his paper of 1928, from which Rosaire gets his data, Lahee speaks of the Mexia structure as the Mexia anticline, of Currie as a fold, and of North Currie as an anticline. In 1928 the importance of the faulting in the Powell-Mexia structures was as well known as it is today and was well known to Lahee. Clapp's and Matson's anticlinal interpretation of the structure causing the accumulation of the gas seems to have been correct. That interpretation was based on more and better data than Deussen's surmise and therefore, superseded it as the scientifically most plausible explanation of the accumulation of the gas; and formed the basis of geologic reasoning in regard to the structures in the area, until yet better data became available.

Those better data did not become available until a considerable number of wells had been drilled to the Woodbine in 1921–22, and then recognition of the importance of

the faulting came with reasonable promptness. But it should be noticed that the accumulation of the oil and gas at Mexia still is to be interpreted as under the control of anticlinal structure. The faulting is important as presumably having produced the anticlinal structure and as modifying it.

Geologists have been guilty of sticking too obstinately to obsolete and preconceived theories based on inadequate data, but Mexia was not an instance of it.

DONALD C. BARTON

PAPERS RELATING TO GEOPHYSICS

Authors' abstracts of papers of geophysical interest offered at the forty-ninth annual meeting of the Geological Society of America at Cincinnati, Ohio, December 29–31, 1936.

3. Geologic interpretation of gravity anomalies in Connecticut and Massachusetts. Chester R. Longwell.

The United States Coast and Geodetic Survey has established 61 gravity stations in Connecticut and Massachusetts, at points chosen with regard to the geology. Nearly half the stations are within the area of Triassic rocks; the others are on the older formations. Specific gravities of rock specimens collected near all stations have been determined.

Systematic differences in the gravity anomalies correspond only in part to known variations in lithology and structure. A west-east gravity cross-section in Connecticut shows a gradual and persistent change from large positive to large negative anomalies, with minor fluctuations related to the local geology. An isoanomaly map constructed from all data in southern New England indicates a large gravity "syncline" plunging southward through Rhode Island, and a broad "anticline" in western Connecticut and Massachusetts. Hypothetical warping of heavy crustal layers at considerable depth, as suggested by Glennie for India, is offered as a logical explanation of the regional distribution of the anomalies.

The Triassic rocks lie in a large graben, and are intruded by thick sills of diabase of high specific gravity. A decrease in the positive anomaly where the line of the section passed from the bordering rocks eastward into the Triassic graben was noted, but there was no corresponding increase in the anomaly at the opposite border of the graben and the anomaly continued to decrease with notable regularity eastward beyond the margin of the graben.

Comment by Dr. Barton was that there was a very evident *regional* gradient of the sort common in the Gulf Coast and that variations should be looked for after its effect had been discounted. He also suggested that the gravity meter, with its greater speed could be used to give many more determinations in the time alloted for field work and that relations to the local geology would doubtless be much more evident with a greater density of stations.

4. Seismological investigations in the western mountain region. N. H. Heck.

Recent rapid growth of earthquake investigations in the United States has, for economic reasons come from those interested in the design of safe structures rather than from the geologist. Because many of the problems are geological, because with more precise knowledge, earthquakes have become important tools for investigating the earth's crust and interior, and because earthquakes are the best evidence that changes are still going on, the geologist will have to take an increasingly active part in earthquake investigation.

The problems can best be attacked through regional investigation like that conducted in southern California, though investigation in the western mountain region cannot be as comprehensive.

A map of all earthquake centers since 1868 is shown, and an appraisal of their accuracy is made. The seismicity is high enough in certain parts of the region to warrant special investigations.

There are only six seismograph stations in the entire region, a totally inadequate number. Instruments for recording strong earth motion have been, and are being installed.

Triangulation and levelling now make possible study of crustal movements. Much more detailed geological investigation is needed. The use of seismic and other geophysical methods in tracing buried geological formations, especially faults, has important possibilities. Gravity observations in increasing numbers may solve problems of earthquake cause.

An appraisal of seismicity and methods of attack, is made for each state in the region.

The author is essentially concerned with the area about Helena, Montana where destructive earthquakes occurred about a year ago. More than two thousand minor quakes have been recorded since that time. Many strong-motion recorders of the type designed by Mr. Brenner (now with I. X. Co.) have been constructed and distributed, and the present paper is intended to stimulate interest in the location and prediction of future earthquakes in the region. A map showing the principal belts of activity was presented.

17. Salt domes related to the Mississippi submarine trough. Francis P. Shepard.

A submarine valley west of the Mississippi passes has been charted in considerable detail by the United States Coast and Geodetic Survey. Unlike the canyons off the east and west coasts, this submarine feature is a broad trough with an essentially flat floor, at places more than five miles in width. This floor slopes outward at a uniform rate of about 2 per cent. The sides of the trough are irregular, apparently as the result of a series of salt domes, which have been intruded around the outer margin of the continental shelf. These domes rise as hills and elliptical ridges one reaching a maximum height of 1,000 feet above the surrounding area. Slopes up to 33 per cent are established on the sides of one dome; others are almost as steep. The inference that these steep slopes indicate rock cores was not proven by such dredging as could be done, but one gentler-sided dome, rising to within 35 fathoms of the surface, was found to have rock fragments on its crest.

The submarine trough is thought to have been cut by the Mississippi River during a low stand of sea level. Its present shape is attributed to a great fill of mud, which is thought to ooze slowly out into the deep part of the Gulf and to have practically obliterated the outer continuation of the valley beyond 500 fathoms.

These soundings were made by the sonic depth-finder, and the greatest detail was mapped along the walls of the canyon described. Its head extends in the direction of Terrebonne Bay.

On the flanks of the canyon, and in a line more than 100 miles long closely following the 100 fathom contour are about twenty or twenty five of the steep-sided conical hills interpreted as salt domes. In plan they are circular to elliptical, one half to two miles in diameter, and spaced five to eight miles apart. No domes were shown on the map between the outer line and the known domes along the shore, except one or two near the head of the canyon.

The Survey has developed a method of determining locations on the water, within a limited range, by means of two or more buoys, accurately located, containing a detector and automatic radio transmitter. A small charge of explosive set off in the water sends out impulses to the detectors in the buoys which return the signal by radio to the ship.

58. Exhibition and description of three-component portable seismograph. Louis B. Schlichter.

A completed sample, of which eleven more are under construction, of a new three-component seismograph and camera will be exhibited and described. The instruments are designed for the purpose of securing accurate ground motions, with special reference to quarry explosions. The design is arranged for changing the mechanical magnification in the range 25 to 25,000; thus the instruments may be adapted to either strong motion or teleseismic use. Results of laboratory tests will be shown.

59. Geologic interpretations of seismic reflection data on the Gulf Coast. H. Rutherford.

Investigation of geologic substructure in the Gulf Coast requires a technique that differs in many respects from that used elsewhere. The absence of definite seismic markers necessitates the correlation of events reflected on the seismogram by either computations or "continuous" shooting.

The various arrangements of the instruments are shown, together with the records obtained. It is also shown how average velocities and dips can be obtained from the data on the records alone. Sample cross-sections obtained by the seismic reflection method are shown.

Papers of general geophysical interest read at the eighteenth annual meeting of the American Geophysical Union held on April 28, 29, 30, 1937 in Washington, D.C. were as follows:

Current geophysical activity in the Gulf Coast, D. C. Barton.

Seismic refraction-methods as applied to shallow subsurface exploration, E. R. Shepard.

Seismology and the geological exploration of ocean-basins, R. M. Field.

Progress-report in seismology for the United States Coast and Geodetic Survey, E. W. Eickelberg.

A preliminary report on the Ohio earthquakes of March 2 and 9, 1937, V. C. Stechschulte.

Progress-report on the research on the Timiskaming earthquake of November 1, 1935, E. A. Hodgson (read by title).

A new theory on the Earth's core, Joseph Lynch.

Intensities of earthquake noises, H. Landsberg.

On the estimation of focal depth from macroseismic data, A. Blake.

A note on land-tilting, J. P. Delaney.

The construction of a low-cost seismograph and recording drum, H. M. Rutherford.

The torsion-pendulum analyser as a double integrator, F. Neumann.

The 23-, 46-, and 92-year cycles in solar and terrestrial phenomena, C. G. Abbot.

Dissemination of data of seven American-operated magnetic observatories, A. K. Ludy, A. G. McNish, S. S. Kirby, and E. O. Hulburt (presented by E. O. Hulburt).

The long-period component of earth-current flow, by W. J. Rooney (by title).

Astatic magnetometer for testing the susceptibility of materials used in magnetic instruments, E. A. Johnson and W. F. Steiner (by title).

Zero distribution-coefficient for horizontal-intensity magnetometers, Geo. Hartnell.

The ionization of the F_2-region, W. M. Goodall.

A study of sudden disturbances of the ionosphere, J. H. Dellinger.

The relation between bright chromospheric eruptions and high-frequency radio transmission fade-outs, R. S. Richardson (presented by S. B. Nicholson).

Radio fade-outs associated with bright solar eruptions, L. V. Berkner and H. W. Wells (presented by H. W. Wells).

Magnetic effects caused by bright solar eruptions, A. G. McNish.

World-wide change in potential gradient, G. R. Wait (presented by John W. Mauchly).

A new approach to the study of solar-terrestrial relationships, John W. Mauchly.

THE SOCIETY ROUND TABLE

MEMBERSHIP APPLICATIONS APPROVED FOR PUBLICATION IN THE MARCH ISSUE, VOL. II, NO. 2, 1937

ACTIVE:

Ralph Frank Acker
N. R. Shade, Chester J. Donnally, Joshua L. Soske
Lem Craddock Bell
Geo. E. Wagoner, R. W. Gemmer, A. B. Bryan
Donald Comnick Bradford
L. L. Nettleton, G. B. Lamb, E. A. Eckhardt
Horvey Cash, Jr.
C. C. Zimmerman, L. A. Scholl, Jr., Alexander Wolf
James Hamilton Ferrick
Walter J. Osterhoudt, R. D. Wyckoff, Paul Weaver
Edward William Fisher
E. Darrell Williams, Ben S. Melton, H. Salvatori
Frank Boyer Gray, Jr.
J. E. Jonsson, Barney Fisher, J. C. Karcher
Paul Frederick Hawley
Joseph A. Sharpe, Harold Washburn, Henry Salvatori
Julius Conrad Heggblom
Leo J. Peters, L. L. Nettleton, E. A. Eckhardt
Harper Harrison Holtzman
M. M. Slotnick, W. M. Rust, Jr., W. B. Lewis
Richard T. Lyons
E. E. Rosaire, T. I. Harkins, A. L. Smith
Charles George McBurney
E. Darrell Williams, Ben S. Melton, H. Salvatori
Edgar Jaffa Meyer
L. L. Nettleton, Leo. J. Peters, E. A. Eckhardt
Adrian Elroy Mix
Wm. M. Barrett, Paul Weaver, F. Goldstone
Hiram Reginald Moorman
Glenn E. Boder, Chas. E. Houston, Dave P. Carlton
Thomas John O'Donnell
Leo. J. Peters, L. L. Nettleton, E. A. Eckhardt
Elton Mayhew Palmer
E. A. Eckhardt, L. L. Nettleton, G. B. Lamb
George Leigh Parkhurst,
Joseph A. Sharpe, Henry Salvatori, Harold Washburn
Wiley Russell Price, Jr.
N. R. Shade, Chester J. Donnally, Joshua L. Soske
Otto Frederick Ritzmann
E. A. Eckhardt, L. L. Nettleton, G. B. Lamb
Gerard William Rulfs, Jr.
H. R. Thornburgh, F. Goldstone, J. Brian Eby

Richard W. Sherman
Frank Rieber, Henry Salvatori, Harold Washburn
Clyde G. Strachan
G. H. Westby, Leo. J. Peters, E. A. Eckhardt
Eugene Hopson Vallat
L. F. Athy, E. J. Handley, Chester J. Donnally
F. A. van Melle
F. Goldstone, H. R. Thornburgh, G. H. Samuels
Charles Edwin Van Orstrand
John H. Wilson, Leland L. Antes, L. Atmar M. Barnette
Louis Niles Waterfall
Chester J. Donnally, Harold Washburn, J. A. Sharpe
Rankin Jones White
J. N. Nahas, Burton McCollum, Wilton W. LaRue
Charles Coburn Williams
E. Darrell Williams, Ben S. Melton, H. Salvatori

ASSOCIATE:

Nathan C. Davies
Phil P. Gahy, Karl E. Lewis, Chester Donnally
Bruce H. Harlton
O. C. Lester, Jr., L. Y. Faust, E. E. Blondeau
Maynard Hugh Jameson
Dart Wantland, C. A. Heiland, A. L. Smith
Ralph Crapo Loring
W. S. Levings, Dart Wantland, C. A. Heiland
Paul McClure Tucker
R. W. Gemmer, A. B. Bryan, Stuart Sherar
Walter Thomas White
Donald C. Barton, J. M. Lohse, M. M. Slotnick
William Jay Williams
Walter J. Osterhoudt, R. D. Wyckoff, Paul Weaver

TRANSFER FROM ASSOCIATE TO ACTIVE:

P. B. Brown, 2011 Esperson Bldg., Houston, Texas
W. B. Hamilton, 2011 Esperson Bldg., Houston, Texas

MINUTES OF THE ANNUAL BUSINESS MEETING OF THE SOCIETY OF EXPLORATION GEOPHYSICISTS

The regular annual business meeting of the Society of Exploration Geophysicists was held at 9:00 o'clock A.M. on March 17, 1937, in Conference Room 1, Biltmore Hotel, Los Angeles, California.

President L. W. Blau presided at the meeting.

The minutes of the Mid-year Meeting in Houston, Texas, November 20–21, 1936, were read and approved.

The Secretary-Treasurer presented his annual report, which was approved.

The President discussed the relations between the Society and the American Association of Petroleum Geologists. The Secretary-Treasurer read resolutions of the Executive and Business Committees recommending amendment of Sections B-2 and B-4 of the Constitution, deleting sections referring to delegates to the Business Committee of the American Association of Petroleum Geologists. On motion duly made, seconded and carried, the submission of the amendments to the Society by ballot was approved.

The President announced the election by the Executive Committee of Mr. L. P. Garrett and Mr. Wallace E. Pratt as honorary members of the Society.

The President announced the election of officers for 1937–38, as follows: President, Dr. J. C. Karcher; Vice-President, Dr. F. M. Kannenstine; Secretary-Treasurer, Miss M. E. Stiles; Editor, Dr. M. M. Slotnick.

On motion made, the meeting adjourned.

(Signed) JOHN H. WILSON

REPORT OF THE SECRETARY-TREASURER OF THE SOCIETY OF EXPLORATION GEOPHYSICISTS AT THE ANNUAL MEETING IN LOS ANGELES, CALIFORNIA, MARCH 17, 1937.

The financial condition of the Society of Exploration Geophysicists as of March 8, 1937, is shown by the following balance sheet:

BALANCE SHEET
SOCIETY OF EXPLORATION GEOPHYSICISTS
March 8, 1937

*Assets**

Current		
Cash on Deposit		
International Trust Company—Checking account	$3,287.60	
National Bank of Tulsa—Savings account	307.55	$3,595.15
Accounts Receivable		
Advertising	24.80	
Subscriptions	64.00	
Reprints	23.11	
Back Numbers	28.44	140.35
Dues Receivable		
1936 Dues	50.00	
1937 Dues	934.00	984.00
		$4,719.50

Liabilities

Current		
Accounts Payable	49.80	49.80
Investment		
Surplus end 1936	2,134.00	
Operating gain to March 8, 1937	2,535.70	4,669.70
		$4,719.50

* Does not include stock of publications.

The operating gain of $2,535.70 which has accrued up to March 8, 1937 has been derived in accordance with the Income and Expense statement shown below:

1937 INCOME AND EXPENSE
SOCIETY OF EXPLORATION GEOPHYSICISTS
to March 8, 1937

Income

1937 Dues Income	$2,400.00	
Income from Reinstatements	10.00	
Subscription Income	213.50	
Sale of Back Numbers	108.45	
Sale of Reprints	53.16	
Total Income		$2,785.11

Expense

Auditing expense	$ 75.00	
Express & Freight	2.92	
Postage	20.25	
Publication expense	5.62*	
Stationery & Office supplies	23.73	
Stenographic & Clerical services	125.00	
Telephone & Telegraph	1.13	
Reprint expense	.62*	
Miscellaneous expense	7.62	
Total expense		$ 249.41
Operating gain		$2,535.70

* Credit

As of March 8, 1937, the Society had a surplus of $4,669.70. Since the income from advertising, subscriptions and sale of back numbers has been sufficient during the past year to pay all publications expense and as additional members in considerable number may be expected during the remainder of 1937, it is fair to assume that the Society will finish the calendar year of 1937 with a surplus of nearly $5,000.00. The Executive Committee thus has funds on hand with which it may enlarge the program of publication, either of GEOPHYSICS or of special volumes, establish reserve funds or scholarships, or carry out any other plans for the betterment of the Society. The Society has maintained a savings account in the National Bank of Tulsa, the present balance being $307.55, for special purposes. This fund could be safely added to, out of the cash on deposit in checking accounts.

During 1936, the Society had a very rapid growth as shown by the following table:

Date	*Honorary*	*Active*	*Associate*	*Total*
January 1, 1936	2	195	34	231
January 1, 1937	2	324	95	421
March 8, 1937	2	367	112	481

With the present flood of applications for membership it is reasonable to assume that the Society will have a membership of 600 or more by the end of 1937.

Observations made during my term of office lead me to suggest that definite educational and experience requirements be set up for membership in the Society rather than leave everything to the discretion of the Executive Committee. Under present

conditions, the members of the Executive Committee are unable to give the necessary attention to investigation of the qualifications of applicants and some members of the Executive Committee are sometimes hesitant about disallowing an application after it has been properly sponsored even though the education and experience record is insufficient to justify membership in a scientific society.

The volume of correspondence and transactions handled by the Secretary-Treasurer has grown enormously with the increase of membership. I consider the amount of work too large to be shouldered by any member who is regularly engaged in other professional work. Fortunately, the Executive Committee has allowed sufficient funds for the employment of part time help to handle the routine business of the Society but with a growing organization there is much that is not routine and must be handled by the Secretary-Treasurer. At some early date, a permanent Secretary-Treasurer should be employed to handle the routine business of the Society. The advantage of a permanent Secretary-Treasurer would be a permanent address for the Society, a continuation of the business of the Society without the confusion and interruption incident to the annual elections, and a reduction of the burdens of the elected officers of the Society.

I give up the office of Secretary-Treasurer of the Society with relief and extend my sympathy to my successor. I have enjoyed the contacts with the membership and with the officers of the Society.

Respectfully submitted,

JOHN H. WILSON, *Secretary-Treasurer*

The program of the annual meeting held at the Biltmore Hotel in Los Angeles, California was as follows:

SEVENTH ANNUAL MEETING OF THE SOCIETY OF EXPLORATION GEOPHYSICISTS

(Formerly Society of Petroleum Geophysicists)

BILTMORE HOTEL, LOS ANGELES, CALIFORNIA

MARCH 15–19, 1937

MONDAY, March 15

2:00 P.M. Registration.

TUESDAY, March 16

9:00 A.M. Registration.

10:00 A.M. Society of Exploration Geophysicists, Executive Committee and Business Committee, joint meeting.

1:00 P.M. Field Trip, Los Angeles Basin.

5:30 P.M. Reception for visiting geologists and ladies. Guests of the Wildcat Committee, California Oil and Gas Association.

WEDNESDAY, March 17

9:00 A.M. Registration.

9:00 A.M. Society of Exploration Geophysicists, Business Meeting. Conference Room No. 1.

10:30 A.M. Technical Session. Conference Room 1.

L. W. Blau, Presiding

1. B. Gutenberg. Geophysics as a Science. 15 min.

The entire science of geophysics is seldom considered as a separate division of knowledge, comparable to geology or geography. Reasons for this attitude are discussed, notably the fact that no one person could hope to master entirely all parts of the geophysical field, or to become proficient in all the branches of specialized knowledge into which the subject, as a whole, may be divided. These subdivisions are discussed, and it is shown that many of them originally developed as branches of some other scientific field from which they have ultimately become separated.

2. A. B. Bryan. Discussion of the Gravity Meter. 15 min.

3. Morton Mott-Smith. Adverse Effects Associated with Variably Compounded Seismic Records. 15 min.

Although the method of variably compounding seismic reflection records, in order to secure so-called "Controlled Directional Sensitivity" has distinct advantages over the more generally used fixed compounding or so-called "Multiple Recording," a simple graphic analysis shows that this "variable compounding" also involves the introduction of adverse effects. Among these are a directional sensitivity less than claimed, the addition of artificial or "machine" noise in the process of compounding, and the magnification of minor waves which the analyzer picks out at random.

This graphic analysis shows that, if the two waves arrive simultaneously from different directions, they can be separated if they differ sufficiently in direction and not too much in amplitude.

Exception is taken to the use of "diffractions" as a term applied to the highly distorted wave fronts reflected from the edge of a fault.

The method is shown to be applicable to special cases, and, probably very few of them, rather than for the general case.

* * * * *

12:00 M. Ladies' entertainment. Luncheon and Fashion Show, Bullock's Wilshire.

12:15 P.M. Geophysicists' Luncheon. Biltmore Bowl.

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Time for the afternoon's Geophysical Session was adjusted to permit members so desiring to attend the Technical Session of the *American Association of Petroleum Geologists* in the Ball Room where the following papers were presented.

1:45 P.M. No. 7. Frank Rieber. Motion Picture Presentation of Reflected Wave Patterns from Various Geological Structures. 15 min.

No. 8. L. W. Blau. The Interpretation of Geophysical Data. 15 min.

* * * * *

3:00 P.M. Technical Session. Conference Room 1.
L. W. Blau, Presiding

4. Marion H. Gilmore. Earthquake Investigations. 15 min.

Seismological work of the Coast and Geodetic Survey began in 1925 with the transfer of the seismological work of the Government from the Weather Bureau. Earthquakes are a menace to life and property, but they are also a source of important scientific knowledge, for they give us information about the interior of the earth which we can obtain in no other way. All earthquakes originate in the outer parts of the earth, but they send out waves which penetrate through the center and can be recorded on delicate seismographs on the other side of the earth. The Coast and Geodetic Survey has developed instruments for recording strong earth motions; has recorded tilt of large areas; has determined dominant periods of buildings, water towers, dams and of the ground; and has made special studies of a statistical nature.

5. J. J. Jakosky and C. H. Wilson. Electrical Prospecting for Oil Structures. 15 min.

This paper describes a new electrical method for oil exploration and gives a brief discussion of the recent results obtained in the field work. Part A of the paper deals with the apparatus and theory of operation, the development history of the process, theoretical factors governing the flow of electric current through rocks, methods of analyzing the field data, and the general field technique. In addition a brief summary is given as to the applicability of the electrical method and its general function in supplementing other geophysical methods for fault location and structure mapping.

Part B of the paper describes various results obtained in the field work, by citing actual cases from surveys which have been conducted wherein the geophysical work has been followed by actual drilling or other development work. Various plates and illustrations are given to show the results obtained.

6. J. A. Sharpe. Magnitude and Character of the Elastic Waves Produced by Explosion Pressures. 15 min.

The solution of the following problem will be outlined: given a spherical cavity of radius a within an elastic medium of density q and compressional wave velocity v, to find the displacement in the elastic wave produced by the application of an arbitrary pressure $p(t)$ to the interior of the cavity. The displacements corresponding to several of the forms of pressure application which may be produced by explosives are illustrated, and the relationship between these solutions of an idealized problem and the actual production of elastic waves for seismic exploration is discussed.

7. Harold Washburn. Transient Characteristics of Seismograph Apparatus. 15 min.

A method of experimentally determining the transient response of seismometer, amplifier, and oscillograph is described. In the apparatus employed the earth motion is replaced by a transient wave generator and the seismometer by an equivalent electrical circuit. Comparison of these laboratory data with field records has shown that accurate predictions can be made from laboratory tests.

8. Lynn G. Howell. Atmospheric Potential Gradient Measurements. 15 min.

Relative atmospheric potential gradient measurements were made in the neighborhood of a salt dome. Two sets of measurements were made simultaneously at two different points. Wulf-type electrometers were used with thorium ionium nitrate collectors insulated with sulphur. A number of set-ups were made and no significant variations in potential gradient were found.

* * * * *

8:30 P.M. Stag Smoker. Rendezvous.

* * * * *

THURSDAY, March 18.

9:00 A.M. Technical Session. Conference Room 1.
J. J. Jakosky, Presiding

9. S. A. Scherbatskoy and J. Neufeld. Fundamental Relations in Seismometry. (Read by Title.)

The equations for dynamic equilibrium of a geophone are derived by means of the Lagrange method and applied to the particular cases of a moving armature geophone, moving conductor geophone, and electrostatic geophone. The galvanometer for for seismic recording is usually of the moving coil type and its equations are similar to those of a moving conductor geophone. The magnetic type geophone as well as the galvanometer consists essentially of two dynamic systems characterized by an asymmetric coupling: "the transducing resistance." A brief analysis is given of the problem of determining the response of a seismograph to a given earth motion.

10. Henry Salvatori. The Mapping of Faults by the Reflection Method. 15 min.

Subsurface conditions associated with certain types of faulting are often such as to preclude the possibility of securing reflections in a region over and adjacent to a fault. Recognition of this fact is important, as a sudden change in the quality of reflections should always be considered as a possible indication of faulting. In regions where reflections can be correlated, faults present little difficulty and the hade as well as the throw of a fault can often be determined. If correlation is impossible and dip shooting

is used the determination of a fault presents an exceedingly difficult problem. In cases where reflections can be obtained continuously across a fault it can be usually detected by continuous profiling. Where, however, a continuous line of reflections cannot be secured, the direct determination of a fault is impossible and its existence can be inferred only after a great deal of close shooting and the consideration of all related factors.

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1:00 P.M. Ladies' entertainment. Trip to Henry E. Huntington Library and Art Gallery, followed by reception at 4:00 P.M. at home of Mrs. Ralph D. Reed, 322 W. Del Mar St., Pasadena.

1:00 P.M. Field Trip, Los Angeles Basin.

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2:00 P.M. Technical Session. Conference Room 1.
H. Salvatori, Presiding

11. D. G. C. Hare. A New Source of Damped Wave Trains Suitable for the Testing of Geophysical Instruments. 15 min.

A newly developed electrical circuit is described, by means of which electrical transients of any desired characteristics may be conveniently produced. Oscillograph pictures of these transient waves before and after passing through elements of geophysical circuits are illustrated.

12. Alexander Deussen and Hubert Guyod. Use of Temperature Measurements in Drill Holes for Cementation Control and Correlation. 15 min.

After comparing the early and the modern methods of temperature investigations in drillholes, the authors review briefly the main applications of these measurements to oil wells. The possibilities and limitations of two specific techniques are discussed, namely: location of cemented zones, and correlations between different wells. The location of the cement behind the casing by thermometric measurements is quite feasible and commercially successful, provided such measurements are made immediately after the completion of the cementation, and preferably before any circulation of mud has taken place. Although they cannot be expected to replace the electrical methods of logging wells, temperature measurements may be used successfully in certain instances to correlate wells already cased where no other method of investigation can be applied.

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8:00 P.M. Dinner Dance. Biltmore Bowl.

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FRIDAY, March 19.

10:00 A.M. Technical Session. Conference Room 1.
L. W. Blau, Presiding

13. F. G. Boucher. The Development of the Pendulum for Prospecting. 15 min.

This paper deals principally with the difficulties encountered in developing pendulum apparatus suitable for geophysical exploration. For convenience it is divided into two main parts, as follows:

I. The pendulum apparatus proper.

II. The auxiliary apparatus.

Under I are discussed the problems of: (a) Maintaining physical dimensions, (b) Eliminating forces on supports, (c) Matching periods, (d) Overcoming air coupling, (e) Holding vacuum, (f) Temperature, and (g) Magnetism.

Under II are discussed: (a) The transmission of pulses from the base pendulum without variable lag, (b) The reception and recording at the field station of the transmitted base pulses, and (c) Workable records.

14. Curtis H. Johnson. Use of the Sonograph in Mapping Faults. 25 min.

Mapping of faults has previously presented great difficulties for the reflection method. These difficulties have often arisen from a confused reflection pattern, rather than from entire absence of reflections near the fault. The use of the Sonograph in making a directional analysis of such confused patterns is described, and illustrated with several examples of faults located and mapped by this method.

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12:30 P.M. Ladies' entertainment. Moving Picture Studio Tour.

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1:45 P.M. Technical Session. Conference Room 1.
L. W. Blau, Presiding

15. M. F. Hasler. The Spectrographic Correlation of Oil Well Waters. 15 min.

A spectrographic analysis of oil well water brines is described, which allows the detection and measurement of a much larger number of elements than the chemical method usually employed, with a considerable saving of time. This larger number of correlation variables has been analyzed statistically and found to give correlations in many cases where the usual chemical tests were inadequate. It is proposed that the same methods may be applied to core analysis.

A detailed discussion by F. Rieber and H. Salvatori was made concerning paper No. 3, by M. Mott-Smith: Adverse Effects Associated with Variably Compounded Seismic Records.

Attention of the members of the Society of Exploration Geophysicists was directed to the following interesting paper, presented at the Technical Session of the American Association of Petroleum Geologists, Ball Room, Friday, March 19th, at 4:30 P.M.

16. E. D. Lynton. Laboratory Orientation of Well Cores by Their Magnetic Polarity.

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SATURDAY, March 20.

8:00 A.M. Field Trip party leaves El Tejon Hotel, Bakersfield, for Kettleman Hills.

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The Geophysicists' Luncheon in the Biltmore Bowl at 12:15 P.M., Wednesday, was tendered to the visiting Geophysicists by a group of California people engaged in Geophysics. A rising vote of thanks was unanimously voted by all the visitors to all those who participated in making the program the splendid success which it turned out to be, and in furnishing the excellent entertainment.

Grateful appreciation was expressed to the following companies whose generous contributions to the General Convention Fund assured the success of this meeting:

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Geological Department, Univ. of Cal. at Los Angeles.—Recent Pacific Sea Shells.
Alexander Anderson, Inc.—Model of a hole with large horizontal drift.
Earl B. Noble.—Collection of Well Cores—California.

RESOLUTION OF THE EXECUTIVE COMMITTEE, MARCH 16, 1937

Resolved: That the Constitution should be amended as follows:

Section B-2: Strike out "and as the junior delegate of the Society on the Business Committee of the American Association of Petroleum Geologists."

Section B-4: Strike out "He shall serve as senior delegate to the Business Committee of the American Association of Petroleum Geologists."

Be it further Resolved: That the legality of the proposed amendments be and they hereby are approved.

RESOLUTION OF THE BUSINESS COMMITTEE, MARCH 16, 1937

Resolved: That the Business Committee hereby approves the amendments to Sections B-2 and B-4 of the Constitution proposed by Resolution of Executive Committee, March 16, 1937; and further, be it

Resolved: That the Business Committee hereby recommends to the Society the adoption of said amendments.